THE MEDICAL NEWS.

A WEEKLY JOURNAL OF MEDICAL SCIENCE.

VOL. LXI. SATURDAY, SEPTEMBER 24, 1892. No. 13.

ORIGINAL ARTICLES.

CHLOROFORM INHALATION.[1]

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I DO not intend to go over the whole of the ground of so extensive a subject as the title of this paper indicates, but rather to present points for discussion, to describe what I have seen of the use of chloroform, and, so far as my observations bear me out, to criticise the report of the second Hyderabad Commission.

Here I would apologize in the words of Dr. Reeves, of Dayton, O., who in discussing the same report says: "In view of some of the names attached to these reports my efforts may be deemed presumptuous. I can only say that the day is past when a name caused acceptance of a doctrine which is not in accord with facts." In order to examine some of the deductions in this report it will be necessary to lay down what I consider the physiologic explanation of the terms "asphyxia," "blood-pressure," "cardiac failure," "syncope," and "shock," for it is to the varying interpretation of these that the differences in the findings of the several commissions on chloroform can be traced.

Asphyxia, when complete, is paralysis of the respiratory center. Its cause may be (*a*) the anesthetic alone, (*b*) the anesthetic plus venous blood, (*c*) venous blood alone. Now, it will be patent to anyone that respiration and circulation are so intimately associated that disturbed function of the one brings about changes in the other.

Is not respiratory embarrassment one of the most striking features of valvular disease? On the other hand, how soon must a muscle like the heart tire when it is supplied with venous blood? A poor circulation is a powerful factor in the production of asphyxia, and the rapidity with which this condition may be brought about, or the success of resuscitation, depends largely on the integrity of the circulatory apparatus.

Blood-pressure is regulated in one of the following ways: 1. By the heart directly, in the force and frequency of its beat. 2. By the heart indirectly, by sending impulses along the depressor nerve to the vaso-constrictor center, inhibiting its action. On this point Foster[1] says: "By means of this afferent nerve from the heart the peripheral resistance is, in the living body, lowered to suit the weakened powers of a laboring heart." 3. Emotions, shock, etc., may inhibit the vaso-constrictor center. 4. Chloroform during the first stage may so excite the nerve-centers that impulses from a heart receiving chloroform sent along the depressor nerve to them can set up changes in their unstable condition far beyond the range of the normal. In this way early syncope undoubtedly develops in some cases. 5. The vaso-dilator center may be stimulated, and the blood-pressure lowered, but it is doubtful if this nerve-center ever plays any part in the production of syncope. I will attempt further on in this paper to explain the cause and method of the constant lowering of blood-pressure under chloroform.

Cardiac failure need not mean necessarily "primary cardiac arrest." Lowering of blood-pressure, dilatation of the heart-chambers and their incomplete mechanism as a force in the circulation, cessation of cardiac pulsations at or immediately following the cessation of respiration, all these indicate cardiac failure as a very important factor in the death of a patient. Cardiac failure, apart from that caused by anesthetics, as happens when a man makes a speech and falls back dead, or when another goes to bed apparently well and is found in *rigor mortis* in the morning, certainly points to the occasional occurrence of this form of death under anesthesia.

Syncope is sudden lowering of the blood-pressure sufficiently to induce stasis of the cerebral circulation and unconsciousness. Any one of the five causes given for decrease of blood-pressure may act strongly enough to produce syncope. The second is the most likely and frequent cause.

Shock, partial or complete, is inhibition of a number of nerve-centers. The vaso-constrictor center is always more or less involved. In this way syncope and engorgement of the abdominal vessels occur. Central shock may come from excessive doses of chloroform. Reflex surgical shock is a result of the reception of painful impressions in the central nervous system. If shock be reflex, it must have a functionally "beneficial intent" in it. I cannot conceive of any such intent, unless the impulse is one of pain. Here one can readily see such intent in producing anemia of the pain-centers, benumbing

[1] Read at the Ontario Medical Association, June, 1892.

[1] Text-book on Physiology, vol. i, page 325, fifth English edition.

them to impulses. It is a well-recognized fact in physiology that stimulation of an afferent nerve when the animal is under the influence of large doses of chloral brings about a fall of blood-pressure quite similar to that caused by stimulating the depressor nerve. If the animal is under the influence of urari, instead of chloral, a rise of blood-pressure follows such nerve-stimulation.

The condition of the central nervous system determines the rise or fall. If the action of chloral is like that of chloroform, then it is possible to have afferent-nerve stimulation (say of the sciatic)—a case of lowered blood-pressure that simulates shock. Syncope pure and simple is very difficult to separate at times from syncope due to shock. If the shock is to be attributed to some particular surgical manipulation, as evulsion of a toe-nail, an incision or crushing a pile, the syncope from such a shock must begin to manifest itself at once. The time taken in a reflex action is usually about one-fifteenth of a second, and it would fall somewhere within this time. Two other points should enter into the decision, *i. e.*, the degree of anesthesia, and the intent or "organized tendency" of the reflex action. Shock will vary with the individual and the degree of anesthesia.

Having thus reviewed at some length the ground in dispute, I will refer to a few points in the report of the second Hyderabad Commission. The first that I am compelled to criticise is that involved in the statement that chloroform does not exercise a depressing effect on the heart in ordinary inhalation. As a result of its experience the commission freely admits that chloroform causes "a gradual fall of blood-pressure." This is the "constant effect," and this fall at times may even "become dangerous." Now a constant fall of blood-pressure must be due to the action of chloroform on the heart or on the nervous system. If, as the commission claims, the drug does not depress the heart, this fall of pressure must be due to its action on the vasomotor centers. Again, if the fall of pressure is due to the action of the drug on the heart, then chloroform must exercise a constant depressing effect on this organ, else why does the latter seek constantly to lighten its load unless to suit its depressed condition. The experiments of the commission were directed wholly to determine if there were a cessation or slowing of the beat of the heart to account for this fall. No effort was made to determine whether it were due to the drug acting on the medulla or not.

Dr. Lewis E. Shore,[1] together with Professor Gaskell, conducted some valuable experiments on dogs and rabbits that can leave no doubt about the cause of this fall of pressure. "In order to decide this question the following experiment was devised: Two animals were taken, and the arteries supplying the brain of one animal were gradually ligatured so that its brain was completely prevented from receiving blood from its own heart. The arteries, carotids or vertebrals, and in some cases both carotids and one vertebral, were then connected to the corresponding arteries of the other animal, and the brain thus supplied with blood, which returned by one pair of jugular veins cross-connected in a similar way. Care was taken that the brain was at no time without a supply of blood. Thus one animal, the *feeder*, was supplying its own brain by one carotid or one vertebral (and this always adequately sufficed), and also supplying the brain of the other animal, the *fed*. The blood-pressures of both animals were taken from the femoral arteries.

"When chloroform was given in the ordinary way, by inhalation, to the feeder, and the usual effect produced in that animal, namely, fall of blood-pressure and slowing of respiration, chloroform-carrying blood was also sent to the brain, and the brain only, of the fed animal, and there produced insensibility, a rise of blood-pressure, and slowing of respiration. When the fed animal, on the other hand, inhaled chloroform, a marked fall of blood-pressure was produced, and as the chloroform could not reach the brain, this could be due only to a direct action of the chloroform on the heart. If the brain of the fed animal, on the other hand, was richly supplied with chloroform by giving large doses to the feeder, the respiration could be brought into a very precarious condition without the slightest fall of blood-pressure, the chloroform depressing the respiratory center, but not the vasomotor."

Surely this throws a flood of light on this vexed question.

The inference is plainly this: That chloroform produces a constant lowering of blood-pressure because of its depressing action on the heart. As long as the result is the same, clinicians do not care whether this is effected by a primary arrest of the beat or by reason of the overtaxed organ calling on the vasomotor centers to let go their hold on the vessels. The heart controls blood-pressure directly or indirectly, and when the pressure is lowered it is because of a demand on the part of the heart itself. This fact having been established, "this depressing action on the heart is a source of danger always," and by a sudden increase of its effect (which might occur from some idiosyncrasy in the patient) primary cardiac arrest might occur. While it is not to be disputed that a lowered blood-pressure will occur in asphyxia, the greatest care should be exercised in accepting this as an explanation of some of the dangerous cases of lessened pressure seen by the

[1] British Medical Journal, November 21, 1891.

commission. It would seem as if the results were obtained too early to have been induced by asphyxia. It is possible that one might get such tracings from the drug and venous blood acting on the heart, when neither alone would yield any results. Careful examination of Professor H. C. Wood's tracings, showing "primary cardiac arrest," with the blood-pressure at zero, will convince any physiologist that Dr. Lauder Brunton is mistaken in his suggestion that these cases were obtained by reason of a clot forming in the manometer.

The next point in the report with which I strongly disagree is the one on "fatty hearts." The commission draw their conclusions entirely from "phosphorus hearts," a fatty degeneration that apparently does not correspond with the slow change resulting from multiple causes, as found in man. I think that I successfully proved last year that elasticity is a vital property of the ventricular wall, and that the endocardium contained as much elastic tissue as the aorta. Now, a few days of phosphorus will not in all likelihood alter the elastic tissue of the endocardium, leaving out of consideration the question whether it alters this tissue before the death of the animal. On the other hand, in many cases of fatty heart in human beings, the endocardium suffers alike with the myocardium, the result being often due to a process of malnutrition. Here, then, we have impaired elasticity coupled with impaired contractility, a predisposing cause to dilatation of all the chambers under the influence of the anesthetic. I deny that the commission had data on this subject from which they could formulate any conclusion. It is almost past belief that they could deliberately assent to the following statement: "Chloroform in no way endangers a fatty heart, although such patients die from very slight exertions or from fright."

Before closing the question of the influence that the heart exercises on the safety of chloroform administration, a few words concerning the cause of such safety to women in labor might be appropriate. To my mind, no small part of this safety is due to physiologic cardiac hypertrophy occurring in the last months of pregnancy. A second factor is no doubt intra-abdominal pressure on "the splanchnic area," preventing sudden syncope.

Leaving the commission and their findings, let me me say a few words about the dangers of chloroform, their causes, and how to treat them. Before doing so, for the sake of convenience in discussion, I will divide the subject into four stages: first, the stage of excitement; second, anesthesia; third, "profound narcosis," fourth, "recovery."

I will classify the dangers as those of asphyxia, syncope, shock, and "reflex cardiac inhibition" (this last may possibly be a variety of syncope, but I wish to speak of it separately).

In the first stage, asphyxia may arise from nasal or laryngeal obstruction. Spasm of the muscles of respiration, I can scarcely believe, ever led to a fatal result. If nasal obstruction is determined before giving the anesthetic, Dr. Butler, of London, Ont., uses cocaine to the parts. This will be more valuable in ether administration on account of the tendency of the latter to produce turgidity of this membrane. In laryngeal obstruction it is best to open the mouth and turn the patient on the side. With the patient in this position I am doubtful about the utility of pulling the tongue forward. The percentage of vapor in the inspired air is a matter of much importance in the eyes of some. I like a Junker apparatus, not because I think the percentage of vapor can exercise a very prominent part in the safety of the administration, but because of the convenience of this apparatus, and there being so little attention needed in the dosage, one is free to look after the patient. If, during vomiting, fear is entertained that some solid matters may enter the trachea, turn the patient on the side and open the mouth, but do not pull the tongue forward. There is a great deal to be said in behalf of the side as the proper position throughout the administration, the Hyderabad Commission to the contrary notwithstanding.

Syncope, as pointed out already in this paper, is common in the first stage, from the unstable condition of the nerve-centers, and it is most liable to happen if the head is elevated; from this come the dangers of operations under chloroform in the dentist's chair. I like to administer the drug without anything under the head; in fact it would be safer if the head were lower than any other part of the body. The treatment for this condition should be to lower the head, and keep it low, and to practise artificial respiration. Hypodermatic injections of digitalin form a valuable adjunct.

Reflex cardiac inhibition. It is a great blunder to think that inhibition will permanently arrest the heart. As Gaskell pointed out six years ago, inhibition means nutrition. The heart soon bounds on again with renewed energy. I have tried repeatedly in the presence of my class to produce death under chloroform by electric stimulation of the vagus, but have always failed. By opening the abdomen of the dog, and passing the hand up between the left lobe of the liver and the diaphragm, the heart could be felt plainly to be inhibited for only a few seconds at a time. I am, therefore, ready to agree with the commission when they say that "the theory that the danger of chloroform administration is slowing or stoppage of the heart by vagus inhibition is now shown to be absolutely incorrect."

The following facts, copied from notes from a typical case of inhibition occurring during teeth-extraction, may be interesting:

"A. B.: took some digitalis and strychnine the night before, and on the morning of the administration I gave chloroform with a Junker apparatus, but could not abolish the conjunctival reflex. I expected reflex inhibition, and asked one of the dentists to take the pulse. He replied, 'seventy, strong and full.' After six teeth were removed I gave him a few more drops to remove three additional ones. I held my finger on the radial artery throughout, and while pulling the last, a lower molar, the pulse suddenly disappeared, the face blanched, the ear applied to the chest of the patient detected no heart-sounds. This continued for about ten seconds, when I felt a return of the pulse, slow and wiry, at the rate of from twenty to thirty. The dentist took the pulse again at my request, and reported 'thirty, and fine as a needle.' The respirations were of a sighing nature throughout. Immediately the pulse ceased, I lowered the head of the patient and awaited events. He now passed into a distinct rigor; the pulse-rate was gradually coming up and he was becoming conscious. He asked for brandy, which was given him (the administration of any liquid is indicated because the act of swallowing abolishes the inhibitory action of the vagus on the heart). I then gave him a few drops of amyl nitrite, with the happy result of flushing the face and dilating the arteries."

It is in this apparently dangerous condition that the drug is useful, and nowhere else in chloroform inhalation.

Second Stage. The leading danger in this stage of anesthesia is the liability to run into the next stage, that of "profound narcosis." Dr. Balfour, of the General Hospital, London, Ont., speaking of this stage, said: "I have given chloroform to cases, one in particular, in which there appeared to be little or no second stage of comparative safety; no sooner would the reflex be abolished than alarming symptoms would show themselves." On the other hand, the majority of cases can stand considerable pushing of the drug after anesthesia, before profound narcosis is reached. In the vast majority of cases the road along which the patient is to be conducted in safety is wide. Rarely is it so narrow that the most skilful will fail to keep thereon. It is wise in some of these latter cases to complete the operation when the patient is in a condition as nearly as possible to incomplete anesthesia.

Third Stage. In profound narcosis, paralysis of the centers presiding over respiration and circulation is what will occur; the center the most frequent to give way is that of respiration. The second Hyderabad Commission apparently obtained nearly all of their fatal results in this stage—a fact that should greatly diminish the importance of their report. The best treatment here is that of hypodermatic injections of strychnine in full doses and artificial respiration.

Fourth Stage. Professor J. A. McWilliams,[1] as a result of some of the most carefully conducted experiments on chloroform, has demonstrated that the heart-chambers undergo dilatation with this drug, even when it is gently administered with plenty of air. He further points out that this may be greatly increased in a pathologic heart. I have myself seen these effects on the hearts of dogs. It appears to result from very incomplete emptying of the ventricles, and has no relation to vagus-inhibition.

Now, when returning consciousness establishes the line of communication between a dilated and laboring heart, on the one hand, and the medulla oblongata on the other, blood-pressure is sure to fall. Urgent messages from an overloaded and distended heart commanding relief often set up violent action in the nerve-centers. This is not to be wondered at when one remembers that these latter are intoxicated. The vaso-constrictor center, receiving cumulative stimuli from the depressor nerve from the heart, releases its hold on all the vascular areas. The patient is bled into his own vessels. The splanchnic flood-gates have opened wide, and the abdominal vessels become engorged. If the heart has been exhausted it will be an unwilling party to receive back the load it so freely threw off. Efforts at vomiting may express large quantities of blood into the right heart and re-distend it. Here syncope will be repeated, probably leading to a fatal result. The treatment is to lower the head, invert the patient, give hypodermatic injections of digitalin and strychnine in full doses, and to start artificial respiration, and keep it up for a long time. Other remedies are of questionable utility, and may contribute to the death of the patient.

There are reports of cases in which the patients died from the effects of chloroform many hours after the inhalation. In Kirk's cases,[2] seventeen hours had elapsed between the inhalation and death. Another is reported "who died some hours after the operation."[3]

At the end of this paper I present notes of the autopsy of one dying thirty-six hours after the administration of the anesthetic. I have clinical notes of one dying three days after. The operation was simple curetting of the uterus, and was done by a skilful specialist for hemorrhage due to a fibroid. It is a fair inference, that on account of the profound anemia that existed for months, there was fatty degeneration of the heart. The symptoms exhibited by the patient were those of "heart-failure."

Shock calls for morphine and heat. Brandy

[1] British Medical Journal, Oct. 11, 18, and 25, 1890.
[2] Ibid., July 11, 1891. [3] Ibid., July 4, 1890.

(after chloroform is very dangerous) can only be useful when given in narcotic doses. While on the subject of alcohol, I might mention that Professor Stephen Smith, of New York,[1] gets his patients thoroughly drunk with whiskey before giving the anesthetic, the patients "frequently jesting and singing on their way to the operation table." He claims that the patient gets quickly under the influence of the drug with a very small dose. He concludes by saying that "in the last ten years I have followed this method of preventing shock, and in no instance has that complication occurred. Alcohol is very much like chloroform in its physiological action, and, given before chloroform, will diminish the required dose of the latter." On the other hand, Professor H. C. Wood[2] has pointed out that in giving alcohol or ether during the stage of recovery, one runs a great risk of poisoning the patient. Alcohol before chloroform is probably beneficial, especially in habitual drunkards, in diminishing the dose of the latter, but given in the stage of recovery is wrong. Morphine and other narcotics, given before the anesthetic is administered, are said to lessen the dose of it.

Circulatory asphyxia. This mixed form of danger, in which asphyxia is brought about by inefficient circulation, is sometimes seen, and should be treated like syncope.

I would like to call attention to a few points among many directions in the giving of chloroform. Wood[3] called attention to the value of digitalis and strychnine in preventing the lowering of blood-pressure under chloroform. I have been using these two drugs before chloroform for the past year; sometimes I have given them for a few days before operation. If I could not administer these drugs six hours before the anesthetic by the mouth then they were given hypodermatically. They keep up a strong, full pulse, and diminish very greatly the "after-sickness" of chloroform.

In two cases recently, after having given strychnine hypodermatically when the patient was fully anesthetized, I discovered that it immediately antagonized the anesthetic effect of the chloroform. To such an extent was this the case that much larger doses of chloroform were required, and great difficulty was experienced in abolishing the conjunctival reflex. I consequently resolved thereafter to use digitalis alone before the administration of the anesthetic, and keep by me a solution of strychnine in a hypodermatic syringe, ready for an accident.

The second point is to watch well the respiratory movements and the face, as by this means one can see the first signs of cardiac failure in the gasping respiration and the pallid face.

The third point is that one ought not to be afraid to let the patient partly recover in intervals of the operation, if there are any such. Roughly speaking, this gives rest to a tired heart.

The fourth point is: Lower the head in all chloroform accidents. Cardiac failure may simulate asphyxia so closely that the haste and excitement will often leave the physicians (attending on the accident) in blind ignorance regarding the exact character of the danger they are treating. The only contra-indication to this inversion is an abdomen distended with ascites or a tumor which by pressure on the diaphragm may dangerously obstruct breathing.

There are cases that hold the elements of danger in themselves, and no care or skill can guard against a fatal result. The danger rests in the "receiver" not the "giver."

CASES OF DEATH.—Mrs. C., forty-eight years old; abdominal section for ovarian tumor and dermoid cyst, in the practice of Dr. Eccles, of London, Ont. She had no alarming symptoms until twelve hours after recovery from the anesthetic, when she appeared to sink from "heart failure," in spite of all medication, dying thirty-six hours after operation. I was asked to make the autopsy. I found the wound and the abdominal contents normal to the naked eye. The peritoneum was smooth and glistening. The right side of the heart held an ante-mortem clot of one ounce; fluid blood filled the right chambers, while the left side was empty; the valves were normal, and all the chambers dilated. The myocardium was pale, and tore easily. The coronary arteries presented well-marked nodular arteritis, and the left was almost obliterated one inch from its origin. On cutting a section of the ventricular wall and throwing it into a dish of water a coat of oil would immediately float on the surface. The microscope revealed well-marked fatty degeneration of the heart-muscle. There was evidence here and in other organs of general arteritis. Is this not a case of fatty heart receiving the fatal strain from taking chloroform?

MR. M., twenty-eight years old, in the practice of Dr. Fulton, of St. Thomas. A student assisted, but the greater part of the anesthetic was given by the doctor himself. It was a clamp and cautery operation for piles. The patient came out from under the drug some time between the removal of the second pile and its cauterization. The clamp remained on the pile for a few minutes, while the operator was in an adjoining room heating the iron. On his return some more chloroform was given to produce insensibility. It was then discontinued permanently. The second pile was now cauterized, the skin about the anus cleaned, and a dressing applied.

Dr. Fulton then examined the pulse, respiration, and face. On finding them normal, he gathered up his instruments, passing into the adjoining room

[1] THE MEDICAL NEWS, Oct. 11, 1890.

[2] Address on "Anesthesia," International Medical Congress, 1890.

[3] Ibid.

to wash them. The student in charge noticed the patient showing signs of recovery. Suddenly the pulse ceased and the face became deathly white. The doctor hastened to the side of his patient and found him trying to vomit. Natural respiration ceased shortly. The tongue was drawn forward and artificial respiration was practised for a few minutes. The head was lowered over the edge of the bed. Natural respiration returned for about one minute. Artificial respiration was resumed, and continued for about twenty-five minutes. Another medical man now arrived with a battery, which was employed with artificial respiration for some twenty minutes, but the patient was dead.

The Crown ordering an inquest, I attended the post-mortem examination at the request of Dr. Fulton. The following are the post-mortem conditions found:

Abdomen: Engorged abdominal vessels; some fluid blood in the cavity (due very possibly to the undertaker's needle wounding a vessel). Venous congestion of the liver (microscopic examination showed the central veins of the lobules and capillaries leading therefrom dilated, as in acute nutmeg liver; also malarial pigment plentifully).

Kidneys: Right, healthy; the left to naked eye showed changes from normal, viz., very imperfectly marked striæ. From one to four drams of milky fluid (which I think was pus) escaped on cutting into the kidney. Microscopic examination discovered round-celled infiltration that in some parts resembled the commencement of small abcesses.

Evidences of the removal of two piles; all the rest of the abdominal contents healthy to the naked eye.

Thorax: The pericardial cavity contained a dram of cherry-colored fluid. The right ventricle was engorged with fluid blood; the left held one ounce or more. The heart itself was "flabby," and all its chambers were dilated. The valves were apparently healthy (the hydrostatic test was not tried). Microscopic examination showed well-marked fatty degeneration of the muscle-fiber.

The *lungs* were engorged (the apices were like the first stage of a pneumonia). On microscopic examination, sections taken from the apex showed the capillaries to be dilated and so injected that at many points rupture of their walls had taken place. By reason of this, many of the air-cells contained blood. The rupture of the capillaries might have been the mechanical effect of the artificial respiration or due to vasomotor influences. Examination of the trachea and larynx was negative.

Brain: The "vessels of the pia mater were injected in one part." The cord was not examined. (I was compelled to leave before the brain was examined by those attending to the autopsy.)

What was the cause of death? Before I answer that, I wish to state that the post-mortem examination alone should not be relied on exclusively. The clinical history of the case (the symptoms the man exhibited when he was dying) and the treatment must have no small part in determining the cause of death. The engorged right heart and lungs are neither constant nor pathognomonic post-mortem conditions of asphyxia.

Respiration is a pump to the right side of the heart, and exercises a powerful suction-force on the veins during respiration. Physiologists[1] are agreed that "were the heart perfectly motionless the working of this respiratory pump alone would tend to drive the blood from the venæ cavæ through the heart into the aorta." Thus, the pumping action of artificial respiration would engorge the right heart and lungs, so that this condition found post-mortem would represent a result of the artificial respiration, no matter what might be the cause of death. We are, therefore, compelled to rely to a great extent upon the clinical symptoms.

The sudden disappearance of the pulse, the blanching of the countenance, the failure to restore by artificial respiration (natural respiration returned for a minute, excluding paralysis of the respiratory center), leave no doubt of the occurrence of syncope in this case.

BARBER'S ITCH.

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THE term "barber's itch" is used by the laity to designate any disease affecting primarily the skin of the bearded portion of the face. The professor of the tonsorial art has been selected as the god-father of a number of distinct diseases whose sole bond of union is in location. This would be unfortunate for the professor, even if it were true that he was the real father and originator of the maladies in question. It is absolutely unjust if, as I think can be shown, the barber has nothing to do with them. He chances to shave a portion of the face prone to be attacked by certain affections, which also commonly occur elsewhere; and, since effect follows cause, and the shaving precedes the itch, therefore the barber causes the disease, and the term remains.

Now, the non-medical mind may be permitted to classify diseases according to their locality. It seems, of late, to have developed an intense hunger for fragments of medical knowledge; and the more indigestible the material with which this hunger is appeased the more likelihood is there of its being rejected or excreted unchanged. The anti-science nature of the ordinary primary and secondary education renders the appreciation of medical facts by the laity an impossibility. To really explain would mean to reëducate them. Since they must have medical knowledge, let them have it in suitable

[1] Foster, vol. i, fifth English edition, p. 219; Landois and Stirling, edition of 1884, vol i, p. 112.

form. Leave them their "barber's itch." But physicians should avoid the term; it is unscientific and meaningless.

It may be an imperfect observation, but my experience teaches me that a large proportion of medical practitioners are not more accurate than the laity in the diagnosis and designation of the diseases of the beard. Barber's itch is still an entity to them; the razor is still the medium of contagion; and arsenic internally is still the means of cure. This is sad, and, what is more important, it leads to non-success in treatment. Yet a little explanation will suffice to straighten out matters.

Three separate and distinct diseases are common upon the bearded face. There are others, but they are of rare occurrence; and others that occur primarily on hairless portions, and may extend to the beard; and still others to which the integument covering the jaws and neck shares a liability with the rest of the dermal covering. These we may leave unconsidered; their rarity and their other symptoms remove them from the category of barber's itch.

The three diseases commonly seen upon the bearded face are:

1. Eczema barbæ, or ordinary eczema.
2. Perifolliculitis barbæ, or sycosis non-parasitica.
3. Trichophytosis barbæ, or sycosis parasitica, or sycosis, or ringworm.

Let us briefly consider their distinguishing features.

1. *Eczema barbæ* does not differ essentially from eczema as it occurs upon other portions of the body. It is a superficial, catarrhal inflammation of the skin, slightly modified by location. It is not necessarily confined to the hairy parts, since the capillary structures are not primarily involved. Hence it may spread from behind the ears, or from the alæ nasi to the bearded face or lip; or, beginning on the hairy skin, it may involve the lips and nose and ears—favorite seats of the eczematous process. The integument affected is reddened and swollen, and secretes abundantly the peculiar gummy serum characteristic of dermal catarrh. The hairs are matted together, and, with the serum, form dry, brownish crusts. In every case, however, the skin itself is intact; a catarrhal inflammation never causes ulceration or destruction of tissue. Removal of the gummy crusts discloses a reddened, inflamed, weeping skin, but no cicatricial tissue or destruction of the hairs. The patient complains much of the itching, as well as of the disfigurement and discomfort caused by the scabs.

2. *Perifolliculitis barbæ* rejoices in as many names as does the heir to a European throne. One of the commonest is sycosis, either alone, or with the adjunct "non-parasitic." This name is to be rejected. It does not describe the disease, and it leads to endless confusion with parasitic sycosis. Both maladies, in fact, are called "sycosis," simply, by different authorities. Perifolliculitis, on the other hand, is descriptive and unmistakable, and it places the malady in its natural relationship with perifollicular inflammations of other portions of the skin.

Perifolliculitis of the beard consists of a pustular inflammation affecting the hair-follicles and the tissues in their immediate vicinity. It therefore commences in the beard, and is strictly limited to it. It begins with the appearance of a few red, conical papules, each of which has a hair implanted in its center. The skin between the papules is unaffected. The cell-exudation soon leads to pus-formation, and we have the characteristic lesion of the disease, an inflammatory pustule pierced by a hair. These hairs, though firmly implanted during the early papular stage, are, of course, loose during the later pustular period, extraction being then painless, the entire hair-sac lying loose in the minute abscess. Not every hair of the area affected is involved, so that there are always healthy tissue and normal hair between the diseased glands. But each hair-follicle affected is destroyed; a minute scar takes its place; other follicles in the neighborhood are subsequently affected; and so, finally, extensive areas may be denuded of hair, and the place of the skin be taken by a thin expanse of cicatricial tissue.

3. *Trichophytosis barbæ* is known as parasitic sycosis (a name to be rejected for reasons already given), and as tinea barbæ, and is simply a ringworm of the beard. It may or may not coexist with ringworm of neighboring or distant parts of the body. When fully developed, its appearance is quite characteristic. The hair of the beard being very deeply implanted, reaching far down into the subcutaneous connective tissue, the growth of the parasitic fungus is not interfered with by the connective tissue of the cutis; it develops freely, and soon leads to the formation of large, deep-seated nodules and tubercles. These masses are characteristic. The surface of the skin may be normal in some places, whilst in others it is fissured, inflamed, or desquamating. The hairs issuing from the nodular masses are broken off close to the skin, and are frayed at their ends, as in ringworm of the scalp. Suppuration around the follicles and permanent destruction of the capillary structures result.

These three maladies, then, constitute the unscientific entity known as barber's itch. The differences between them are usually so marked that the diagnosis is easy.

How are they to be avoided? If the hygiene of the barber-shop were all that is necessary, our task would be an easy one.

Eczema of the beard is in no sense a contagious

disease. It has never been caught by one patient from another suffering from it; it is not inoculable. Yet more than half of the cases of barber's itch are cases of simple eczema. Neither the barber nor any of his customers has anything to do with it.

Eczema of the beard, like eczema elsewhere, is due to certain external and internal conditions. External irritants, chemical or mechanical, will cause it; and an acrid nasal discharge is responsible for a considerable number of eczemas of the upper lip. The internal causes are not very definite; but it is agreed that dyspepsia and inefficient renal action predispose to dermal catarrhs.

The avoidance of this form of barber's itch is, therefore, dependent upon the avoidance of certain external irritants and the care of the general health. Improper—*i. e.*, too strongly alkaline—soaps are especially to be avoided, and the digestive and renal functions should be kept in order.

Nor is perifolliculitis of the beard a contagious disease, save in the limited sense that all pus may be contagious under certain conditions. Irritating powders and cosmetics, exposure to dust, or to intense heat, are the causes of the malady, which is especially likely to occur in those in whom the hair of the beard is stiff and wiry. A localized or mild eczematous condition often causes so much irritation that the finger-nails are freely used, and by these foci of purulent infection pus-organisms are implanted in the hair-follicle. Thus eczema and perifolliculitis frequently coexist, but the predominance of one or the other morbid process is generally marked.

The prophylaxis of perifolliculitis consists, of course, in the avoidance of the agencies that cause it. The face-powders used after shaving are undoubtedly responsible for some cases. Thus, again, in this second variety of barber's itch the barber is entirely innocent.

Ringworm of the beard, the third and last variety, is a frankly contagious disease. Whether or not there is more than one variety of the trichophyton parasite is a question still under discussion; but ringworm of the beard is always derived from another case of ringworm, and this malady is sometimes carried from one person to another by the barber. I say sometimes; I might say rarely, for the chances of contagion in this manner are small. The razor itself touches the skin only with its edge, which is frequently wiped and stropped; the fungus could hardly adhere to its blade. The lather-brush, the towels—but especially the barber's hands and finger-nails—are the mediate carriers of the contagion.

The prophylaxis is cleanliness; boiling of the towels; thorough cleansing of brushes, or, better still, individual brushes and cups; the use of the nail-brush on hands and finger-tips after each shave; with these ringworm of the beard may surely be avoided. There has been improvement in the hygiene of the barber-shop of late years, but enough cheap and careless ones remain to maintain in our dispensaries and clinics a steady, though small, stream of cases of trichophytosis barbæ.

And now as regards the cure of these diseases of the bearded face. They are frequently obstinate, prone to relapse, and annoying to the physician. An accurate diagnosis is necessary, for in certain essential particulars the treatment required differs in these three maladies.

Certain general rules may be laid down. The general health must be cared for; the gastro-intestinal and renal functions must be examined and regulated; sources of irritation—bad soaps, dusting-powders, exposure to dust and to the inclemencies of the weather—must, if possible, be eliminated. Nasal and aural discharges must be cured.

Treatment of Eczema of the Beard.

1. *Dietetic.* Put patients of full habit, hemorrhoidal tendencies, and prone to excesses at the table, upon a carefully regulated diet, mainly vegetable, with meat but once daily, and no alcoholics or coffee or tea. In obstinate cases a milk and farinaceous diet exclusively gives brilliant results. On the other hand, anemic patients should be put on a full, roborant diet.

2. *General.* Saline purgatives should be freely used in the plethoric cases, especially in the form of the natural waters. Anemic cases should have tonics, bitters, cinchona, iron, and strychnine. Alkaline diuretics are indicated in almost all cases, as:

℞.—Potassii citrat. . . . . gr. xx.
Infus. tritici repentis . . . ℥ss.
Infus. digitalis . . . ℥ss.—M.
S.—Twice daily.

Or,

℞.—Potassii acetat. . . . . gr. xx.
Spts. æther. nitrosi . . . ℥ss.
Syrup. aurantii . . . . ℥ss.—M.
S.—Twice daily.

Both are to be given in large quantities of water. Arsenic is useless, even in chronic cases.

3. *Local* treatment is most important. Remove all crusts by soaking with olive oil, and, if necessary, by means of soap and warm water; but thereafter, as much as possible, avoid the use of water, which is irritant. Cut off the hair of the affected part to facilitate treatment. Apply one of the following ointments:

℞.—Bismuthi oxidi . . . . ℥j.
Glycerini . . . . ℥ss.
Ungt. aquæ rosæ . . . ℥j.—M.

Or,

℞.—Zinci oxidi . . . . ℥j.
Acid. oleici . . . . ℥ss.
Petrolati . . . . . ℥ss.—M.

Zinc paint affords a thorough protection, and is very soothing:

℞.—Zinci oxidi . . . . ℥ij.
Mucil. gum. arabic. . . . ℥ss.
Glycerini . . . . ℥ss.—M.
S. Apply with a brush.

In the later, chronic stages, use the diachylon ointment; if the scaling still persists, tar or resorcin must be employed, as:

℞.—Ol. cadini . . . . ℥ss.
Pulv. zinci oxidi . . . ℥ij.
Amyli . . . . . ℥ij.
Adipis . . . . ad ℥j.—M.

Or,

℞.—Resorcini . . . . gr. xv.
Lanolini . . . . ℥j.—M.

Treatment of Perifolliculitis Barbæ.

In a purely local disease, dietetic or general treatment is not necessary, save in so far as applies to the removal of any irritant—cosmetics, snuffs—that may seem to have some etiologic relationship to the malady. Cod-liver oil given internally seems, as in so many other affections, to control the tendency to pus-formation; it should be given freely in obstinate or extensive cases. Calcium sulphide is absolutely useless.

Local treatment. The beard or mustache must be removed from the area affected, as it interferes with treatment. Shaving is liable to further irritate the skin and to cut off the hair so short that it buries itself in the inflammatory papule and cannot be extracted. Have the hair simply cut close. Now epilate every affected hair in the area. If there are too many for one sitting, take them in sections. Hair-bulbs surrounded by pus are loose, and can be removed without causing pain; the proceeding is as rational as to open a furuncle and to remove the splinter of wood that causes it. Even before suppuration has set in, every hair that is evidently the center of inflammatory action should be pulled out. By this means a chief source of irritation is removed and free exit is afforded the sero-pus. In this way, destruction of the hair-follicles and their surroundings, as well as baldness and scarring, are best avoided.

For this purpose a forceps with flat, unserrated blades is more convenient than the ordinary kind Each hair must be extracted singly, traction being made in its axis. The operation is painful, but necessary; it should be repeated at intervals by the physician, since neither the patient nor his friends will have sufficient skill and fortitude to do it themselves.

The way being thus prepared, soothing ointments are to be used. A favorite of mine is the following, which is sedative and very bland:

℞.—Tinct. opii . . . . ℥ss–℥j.
Acid. carbol. . . . . gr. x.
Ungt. aquæ rosæ . . . ℥j.—M.
S.—Apply twice daily; keep on constantly.

A little later, when the first acute stage has passed, astringent applications are in order. Thus:

℞.—Ungt. diachyli . . . . ℥ss.
Ungt. zinci oxidi . . . ℥ss.—M.

Or,

℞.—Hydrarg. chlor. mit. . . . ℥ss.
Petrolati . . . . . ℥j.—M.

Finally, the following may be employed:

℞.—Ichthyol. . . . . ℥ss.
Lanolini . . . . . ℥j.—M.

It is well not to forget, and not to let your patient forget, that perifolliculitis of the beard is frequently a very obstinate disease.

Treatment of Trichophytosis Barbæ.

Here, again, general measures are useless.

Local treatment. Remove the crusts with olive oil, warm water, and soap; then get rid of the excess of hair by cutting—not by shaving—for the reasons already given.

If epilation of the *affected* hairs is required in perifolliculitis, *all* of the hairs must be removed in ringworm. It is impossible to tell with certainty which hairs are healthy and which are diseased. Hence every hair of the affected area and for some distance around it must be pulled out. The process is painful, for the hairs are often firmly implanted; nor does cocaine, either applied externally or injected, help matters much. The pain must be borne, for without removing the hair and opening up a route for our remedies no parasiticide can penetrate to the depths of the follicle in which the mass of the fungus is growing.

Even after epilation it is sometimes difficult to find an agent strong enough. Go at once to:

℞.—Hydrarg. chlorid. corrosiv. . gr. ij–iv.
Glycerini . . . . . ℥j.
Aq. cologniensis . . . ℥j.—M.
S.—Apply twice daily.

Start with the weakest solution, and rapidly increase its strength. For small diseased areas emplastrum hydrargyri or unguentum hydrargyri does fairly well. A solution of the oleate of mercury—from 5 per cent. to 10 per cent.—in oleic acid is of use, and should be painted on twice daily. But in almost all cases we are finally compelled to have

recourse to mercuric chloride, and we might as well start with it. A twelve-grain-to-the-ounce solution has proved necessary in some few cases; of course, such strong solutions must be applied by the physician himself; they cannot be entrusted to the patient.

25 WEST FIFTY-THIRD STREET.

THE PRODUCTION OF IMMUNITY IN GUINEA-PIGS FROM HOG-CHOLERA BY THE USE OF BLOOD-SERUM FROM IMMUNIFIED ANIMALS.[1]

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THE important results obtained by Behring and Kitasato, Tizzoni and Cattani, Klemperer and Klemperer, and others, with diphtheria, tetanus, pneumonia, etc., led me to undertake a series of experiments upon guinea-pigs for the purpose of studying the effect of the blood-serum from animals immune to hog-cholera. As a source for the blood-serum, I used guinea-pigs that had been immunified by means of the albumose extracted from artificial hog-cholera cultures, and then exposed to the disease by inoculation with the virulent germ.[2] From this latter inoculation the animals had not suffered, and at the time of use had been well for several months. The blood was drawn from the carotid artery into a sterile test-tube, and defibrinated by means of a sterilized glass rod, the overlying serum then being used for injection. Precautions were taken that the blood did not become contaminated while being drawn, and the absence of germs from the serum was carefully tested.

The experiments were as follows:

Exp. 1. Two guinea-pigs, weighing each about one pound, received three c.c. of this blood-serum by a single injection on the inner side of the thigh. This injection was followed by a slight rise in temperature, but no local lesion at the point of injection, though there was a slight inflammation of the skin for four or five days. After ten days these pigs, together with two checks, were inoculated in the thigh with $\frac{1}{10}$ c. c. of peptonized beef-broth hog-cholera culture, one day old. The checks died in seven days. The autopsy showed characteristic hog-cholera lesions; the hog-cholera bacteria in the spleen were abundant. Both treated animals apparently recovered from the inoculation. Four weeks after the inoculation one of the pigs became paralyzed in the hind legs, and finally died five weeks after the inoculation. The autopsy showed the organs to be apparently normal, except for a few small dots on the surface of the liver. Culture showed an absence of hog-cholera germs.

The second treated pig was found dead three months after the inoculation. The autopsy again showed the organs apparently normal, except for a slight injection of the intestines.

There had in these two cases been then a considerable resistance to the disease from a single injection of the blood-serum. As the germ could not be detected by cultures, and the organs, further, were apparently normal, it is probable that some other cause than hog-cholera caused the death of the animals.

Exp. 2. The experiment was repeated upon exactly the same number of animals, and in the same way, with the result that the pigs treated with the blood-serum recovered entirely, while the checks died. The autopsies of the checks showed characteristic hog-cholera lesions and the germs were abundant in spleen and liver. The experiment repeated again gave the same results—a recovery of the treated animals and the death of the checks.

This work, taken in connection with my earlier results in immunifying guinea-pigs,[1] made it apparent that the blood-serum was a potent factor in the production of immunity.

While in the case of the use of the albumose extracted from the artificial sterilized cultures, or in the use of the simply sterilized cultures, from ten to fifteen c.c., or the equivalent, were required for immunization, here only three c.c. of the serum were necessary.

Treatment of Guinea-pigs Inoculated with Hog-cholera with the Serum from Immunified Animals.—I next tried to cure guinea-pigs with the blood-serum:

Several pigs were taken, and all inoculated with $\frac{1}{10}$ c. c. hog-cholera culture, one day old. Two days afterward two of these animals received an injection of 1.5 c.c. of the blood-serum from an immunified animal. The other animals were reserved as checks. The injections with the same quantity of serum were repeated after two days. The result of the experiment was that while the checks died within ten days, the treated animals resisted for from a week to ten days longer, finally, however, succumbing to hog-cholera, as proved by the autopsies.

The experiment was repeated with the same quantity of material, and in the same way, and I succeeded in saving one of the animals, the recovery being entirely satisfactory. All of the checks died. This cure may have been accidental.

At present I am making a more extended series of experiments in this line, beginning the injections with the blood-serum within twelve hours after the infection, and repeating them more frequently. In

[1] Read before the Association of Acting Assistant Surgeons, Rochester, August 15, 1892.

[2] THE MEDICAL NEWS, October 4, 1890.

[1] THE MEDICAL NEWS, October 4, 1890.

this way it may be possible to effect a cure in every instance.

Just at this point in my experiments the copy of Pasteur's journal (*Annales de l' Institut Pasteur*, May 25, 1892,) reached my hands, in which a series of experiments upon rabbits is described by Metschnikoff, with the germ which he considers to be identical with that of our hog-cholera. His results were the same as those I had obtained, an easy immunization by means of the blood-serum. Metschnikoff's injections of the blood-serum were all intra-venous and repeated several times. My injections were subcutaneous. The practical results, however, in both cases were the same.

A few tests were made in order to see if the action of the blood-serum was due to its germicidal properties. These experiments showed that the serum *outside of the body* did not exert a germicidal action on the hog-cholera bacillus, and on this point they also agree with Metschnikoff's work.

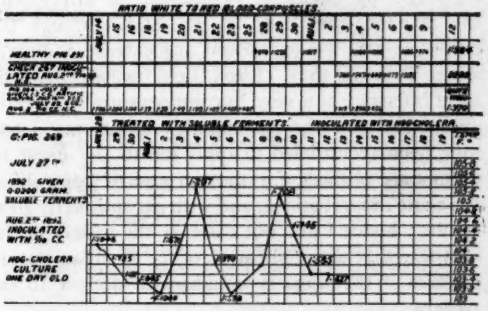
At the same time that the blood-serum experiments were being carried on I had a number of guinea-pigs that were being immunified with the albumose extracted from milk-cultures, the products from purely artificial mineral salt and glycerin-cultures, and with the soluble ferments that I have recently obtained from the hog-cholera cultures.

In view of the observations of Roemer[1], Neisser[2], Canon,[3] Botkin,[4] and Tschistowitsch,[5] I thought it would be of interest to learn if there was a noticeable increase in the number of white corpuscles in the blood of guinea-pigs inoculated with the germ alone, and in the animals while being treated and after treatment, when inoculated.

The blood was taken daily from the ear of the animal, and the hemocytometer of Thoma-Zeiss employed for estimating the number of corpuscles. Toison's solution served for the staining fluid. Beginning twenty-four hours after each injection or inoculation, the counts were conducted till the animals either died or recovered from the disease. To serve as a check upon the number of white blood-corpuscles, a healthy animal was also used. Comparing the figures thus obtained, allowing for natural daily variation and individual error in counting, we find an increase in the number of white blood-corpuscles, both in the checks and in the treated animals. After the first injection for immunifying the animal, the white blood-corpuscles apparently increased, and then returned to the normal as the animal became well. When these same animals were inoculated, the increase of the white corpuscles and return to the normal were equally marked. In the checks, those inoculated with the germs showed a decided increase, and then decrease in the number of white blood-corpuscles, till the animal died. These results, though but preliminary as regards the disease of hog-cholera, may be considered as throwing some light on Metschnikoff's theory of phagocytosis.

While writing this article, Kanthack's[1] paper reached my hands. He reports observations upon the leukocytosis produced by the pyocyaneus-cultures, and sterilized cultures of the vibrio Metschnikovi. As his injections were made intra-venously and with fairly large amounts of the material, the increase in the number of leukocytes was more rapid. He observed, upon repeated hourly examination, first a decrease and then an enormous increase in the number of white blood-corpuscles. In my experiments this increase was not quite so marked, nor was the decrease at first noticed, but as the counting of the corpuscles was not begun till twenty-four hours after the injections, it would not have been observed.

In the appended chart is represented the relative number of white and red corpuscles, as found in a healthy guinea-pig, No. 231, in a check, No. 267, and in two treated and then inoculated pigs, Nos. 224 and 269. In the case of No. 269 the corresponding temperatures are also noted. I may take occasion here to acknowledge the help of my assistant, Mr. Emery, in the estimation of the leukocytes.



The results of my work show that there is an easy immunization of guinea-pigs from hog-cholera by means of the blood-serum of immunified animals, and that there is a possible cure of infected animals by a similar treatment. Further, they point to an apparent education of the white blood-corpuscles in the blood of guinea-pigs by means of the bacterial products.

The work agrees practically with that of Metschnikoff upon rabbits.

[1] Berliner klin. Woch., 1891, No. 36.
[2] Wiener Med. Presse, 1892, Nos. 3, 4 and 5.
[3] Deutsche med. Woch. 1892, No. 10.
Ibid., 1892, No. 15.
[5] Berl. klin. Woch, 1891, No. 34.

[1] Brit. Med. Journ., June 13, 1892.

CAN CROUPOUS PNEUMONIA BE ABORTED?

BY THOMAS J. MAYS, M.D.,
PROFESSOR OF DISEASES OF THE CHEST IN THE PHILADELPHIA POLYCLINIC, AND VISITING PHYSICIAN TO THE RUSH HOSPITAL FOR CONSUMPTION.

IF one fact is more patent than another, it is that when the medical profession comes to deal with the treatment of croupous pneumonia it is like a rudderless ship in a storm at sea. Why this should be so is not easy of explanation. Here is a disease, constant in its pathology and morbid anatomy, and the physical signs and nature of which are well recognized, and yet the most opposite and varied forms of treatment are and have been advocated, and it is even taught by some of our latest text-books on practical medicine that the expectant or "let alone" treatment is productive of the best practical results.

This seems to be a startling state of affairs, although I must confess that until recently I had no special reason to believe that my work in this direction was followed by any better results than were to be expected by natural recovery from the disease. From later experience, however, which, I trust, will not prove altogether anomalous, I feel convinced that the skepticism that has invaded the profession concerning the utility of therapeutic art in pneumonia is not justified by what may be accomplished. The ground for this faith rests principally on the following detailed results that have been obtained in the treatment of two cases of acute pneumonia under my care in the Polyclinic Hospital. The principal treatment in these cases—which was, I believe, first suggested by Niemeyer—consisted in enveloping the affected chest-area in ice, and, although the cases are too few to warrant any extensive deductions, I think they clearly demonstrate that the local influence of a very low temperature is directly antagonistic to the pneumonic condition. I had long been accustomed to apply ice to the chest in pulmonary phthisis, for the purpose of controlling the acute exacerbations, and with exceptionally good results; hence I did not share in the widely prevalent fear that the application of ice in inflammatory diseases of the lungs is fraught with danger to the patient.

CASE I.—P., a colored female, twenty-five years old, came under my care November 24, 1891, with the following history: Four days previously, when her temperature, pulse, and respiration were normal, Dr. Baer removed her left ovary, which contained a large hematoma. On the morning of my first visit she had a temperature of 103.4°, and a physical examination showed that the lower parts of both lungs were in a state of consolidation, extending the higher upon the right. There was also evidence of pleurisy on this side. The temperature had risen rather suddenly, for on the morning of the 22d it was 99°, while on the evening of the 23d it reached the height of 103.4°, as already stated. On the morning of the 24th ice-bags were applied to the front of the chest upon the right, and to the lateral aspect of both sides of the chest, and, with few intermissions, were kept almost constantly applied for about two weeks. The temperature began to decline immediately after the ice was applied, and, excepting a slight rise on December 3d, sank to nearly a normal level on the eleventh day, as is shown by the following temperature table:

	Morning.	Evening.
November 24th	103.4°	102.4°
" 25th	101.4	100.6
" 26th	100.0	99.8
" 27th	101.0	100.0
" 28th	100.8	100.8
" 29th	99.0	101.0
" 30th	101.0	99.8
December 1st	99.2	99.8
" 2d	99.6	99.8
" 3d	98.8	101.4
" 4th	98.5	99.8
" 5th	99.2	99.8
" 6th	98.6	99.8

Subsequently, the temperature remained practically normal.

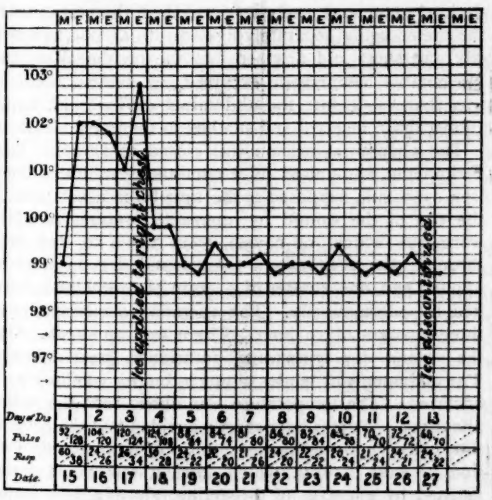
Simultaneously with the fall of the temperature there was a corresponding improvement in the physical signs and symptoms. The depressing effect of the ice on the temperature-curve was immediately shown after its application. When the ice was removed, as it was several times during the course of the disease, the temperature almost invariably rose. This partly accounts for the unsteady temperature-line between morning and evening, which is not shown fully in the foregoing figures—although, on the whole, the woman was of a nervous temperament, and this unquestionably influenced her temperature-curve in a measure. Her nervousness is indicated by the frequency of her respirations; these were 48 per minute at one time, and in four hours they fell to 24, and without any apparent cause.

In addition to the ice she received two ounces of freshly-expressed beef-juice alternately with half an ounce of beef-meal in coffee or chocolate, every two hours, and half an ounce of whiskey in a glassful of milk, every three hours. As far as drugs were concerned she received quinine and phenacetin internally in small doses, and an occasional hypodermatic injection of morphine to produce sleep and to alleviate pain.

In some respects this case was peculiar. There was no cough or expectoration, except on the seventh day, when several mouthfuls of blood were spat up. The woman suffered greatly from dyspnea, although the pulse never exceeded 120 in frequency, and usually oscillated between 90 and 106. It may be said that the defervescence was slow, but when it is remembered how prone any inflammatory

disease of the lung in the negro race is to run into catarrhal disease and become chronic, I think the result on the whole is rather gratifying.

CASE II.—F., a girl, eighteen years old, was first seen on June 17, 1892, when she presented the following physical signs: The pulmonary percussion-resonance was impaired over the whole anterior surface of the right lung, with dulness, approaching to flatness, over the whole posterior and lateral surfaces of the lung. Expiration was decidedly prolonged over the anterior surface from apex to base, and crepitant, subcrepitant, and some sibilant râles were heard over the posterior and lateral portions of the lung. The conditions manifested on the part of the left lung were normal, except that the breathing was puerile. There were abdominal pain, cough, mucous expectoration, and a temperature of 102.8°; the respirations were 34, and the pulse 124. On the afternoon of this day the right chest was well enveloped in ice-bags—a large one in front, and two at the side and posteriorly as much as possible. Another ice-bag had been applied to the head in the morning. Internally the girl received $\frac{1}{25}$ of a grain of strychnine and five grains of quinine every three hours. She was also given an ounce of fresh beef-juice every two hours, and milk and whiskey. On the morning and evening of the following day (18th) the temperature had fallen to 99.8°, and the physical signs were unchanged, except that the prolonged expiration had disappeared anteriorly and was replaced by crepitation. On the 19th the sputum was rusty-colored, and in the evening the temperature



was 98.8°. On the 21st there was a perceptible improvement in the pulmonary percussion-resonance of the whole lung. Very few râles were heard, except at the extreme lower part of the lung. The expectoration had become muco-purulent and rusty. On the 24th no râles could be heard at any part of the chest. The percussion-resonance was vesicular anteriorly and laterally, and but slightly impaired posteriorly. Expectoration had ceased. Strychnine was discontinued on account of a manifestation of its physiologic effects. On the 26th the percussion-resonance continued to be slightly impaired in the right interscapular region. No râles were heard. The respiratory murmur was normal over the whole lung, excepting for slight impairment in the interscapular region.

On the 27th the ice was discontinued, and on the 6th of July, after progressive convalescence, the girl was discharged. The chart illustrates the temperature, pulse, and respiration of this case.

In conclusion, I do not desire to be understood as believing that ice will be found a panacea in pneumonia, but I think the histories of the two cases here related show that cold applications to the chest have a direct, beneficial action on the pneumonic process, not only in the sense of limiting its extension, but in dissipating it. This was especially well demonstrated in the second case, in which the local condition, as indicated by the physical signs, improved immediately after the ice had been employed.

One other feature may be referred to in this connection. In neither of these cases was there anything like a critical period, but merely a more or less rapid subsidence of the local and general disturbance—giving one the impression that, if the disease was not aborted, it received a decided check, and was certainly not allowed to pursue an even and uninterrupted course.

Probably not the least important lesson that may be learned from these cases is that the fear that cold or ice applications in pneumonia are detrimental to the patient is entirely unfounded, and, if this demonstration will serve to allay this apprehension, I feel that these cases have not been reported in vain.

CLINICAL MEMORANDA.

A CASE OF ABDOMINAL NEPHRECTOMY:

With Practical Lessons Thereon.[1]

BY SCHUYLER C. GRAVES, M.D.,
VISITING SURGEON TO ST. MARK'S AND THE U. B. A. HOSPITALS, GRAND RAPIDS, MICHIGAN.

IT is not my purpose to present an exhaustive paper on the subject of nephrectomy. I desire, on the contrary, to report, simply and briefly, the history of a successful case that has occurred in my practice.

Let the fact, then, that the operation is one of considerable interest to surgeons, on account of its comparative rarity and the high mortality-rate with which it is associated, be my excuse for making this report.

The case presents several features of peculiar interest in pointing a moral that, I trust, may spare others many moments of anxiety, which it has been my unfortunate lot to experience.

[1] Read before the Surgical Section of the Michigan State Medical Society, at Flint, Michigan, May 5, 1892.

Mrs. C. S., twenty-nine years old, eight weeks after the birth of her only child, a boy of about seven, noticed a lump in the left side, as large as the palm, and not painful except upon pressure.

Dr. Martin, the first physician consulted, saw the case one and one-half years after the tumor had been discovered. He came to the conclusion that the tumor was splenic, and prescribed tonics and applied the galvanic current.

The patient states that the tumor seemed to grow smaller during the year of current treatment, but increased in size as soon as the treatment was withdrawn.

The patient then had no particular treatment for about two years, at the expiration of which time she consulted Prof. McGraw, at Detroit. Dr. McGraw made a diagnosis of enlarged and dislocated spleen, and recommended non-interference. He declined operating and advised the patient to return home, giving her, at the same time, some medicine which he said he hoped would check the growth of the tumor. He did a perineorrhaphy upon the patient, however, before she left the Detroit hospital, in the hope of building up a somewhat shattered constitution. This was about two years ago.

Time went on, and no particular change was noticeable in the tumor or in the general health.

In January of the present year the patient fell into the hands of Dr. B. J. Zudzense, of Sparta, Kent Co., Mich., who kindly referred the case to me.

During all of these years the health of Mrs. S. had been feeble; constipation had been the rule; the appetite was at times poor and then abnormally great; she slept badly for weeks at a time; the menstrual functions were normal.

There was no suspicion on the part of the patient that any renal trouble existed; she seemed to pass as much water of normal character as ever. At times the tumor was painful.

When I first saw Mrs. S. she presented a somewhat cachectic appearance, though not at all suggestive of malignant trouble. She was thin and tall ("long for this world," as she expressed it); her weight was in the neighborhood of one hundred pounds.

A solid, movable tumor about the width, length, and shape of an adult hand, lay within the abdominal cavity, on the left side under the linea semilunaris and opposite the umbilicus. The tumor could be pushed upward almost completely behind the ribs, and was also quite freely movable laterally, as well as downward. The abnormality was evidently one of five things: 1. A floating spleen, diseased or not. 2. A diseased and dislocated kidney. 3. A benign neoplasm, or an inflammatory thickening of walls of the colon. 4. Fecal impaction. 5. A tumor of the omentum.

Repeated and unavailing bowel-washings, with large quantities of warm water twice daily, excluded the possibility of fecal impaction, and the analysis of the urine seemed, except in one particular, to indicate no trouble with the kidneys, as the following report will show:

Urinalysis. Mrs. C. S., January 25, 1892 (three days before operation).

1. Quantity (in twenty four hours), thirty ounces.
2. Specific gravity, 1020.
3. Color, amber.
4. Odor, normal.
5. Reaction, acid.
6. Albumin, absent.
7. Sugar, absent.
8. Deposits, absent.
9. Microscopic examination, scattered epithelial cells from the bladder.

The exception referred to is the twenty-four hour quantity, being thirty ounces instead of forty-eight. This is merely a relative matter, and would not excite suspicion unless it proved to be a constant factor.

No evidence of leukocythemia was present.

The woman was anxious to be relieved. She was informed that one of the five conditions already noticed existed; fecal impaction was excluded; a positive diagnosis could only be speedily and accurately made by opening the abdominal cavity and exposing the tumor to sight and touch.

To this proposition the patient readily assented, with the understanding that should anything be found the removal of which would prove extremely dangerous to life, it would be left *in situ*, provided, of course, that its removal was not rendered necessary in order to save her life.

Under these circumstances the operation was undertaken on January 28, 1892, at the U. B. A. Hospital.

After the usual antiseptic preparations, the abdominal cavity was opened by an incision, four inches long, in the left linea semilunaris (Langenbeck's incision) and the tumor was soon exposed. It was almost immediately recognized as the left kidney, which had undergone multiple cystic degeneration. Notwithstanding its previous mobility, a goodly number of adhesions were encountered in its removal, which, however, was quickly decided upon and easily accomplished, the vessels and the ureter being separately ligated with aseptic, braided silk, cut short and dropped into the peritoneal cavity. No notable hemorrhage occurred. A lumbar incision was then made into the abdominal cavity and a large rubber drain inserted and stitched to the integument.

The abdominal incision was closed as follows: The peritoneum was united by a continuous suture of fine, iron-dyed silk, supposed at the time to be aseptic; and the rest of the wound by interrupted sutures of silkworm-gut, which, of course, went down to, but not through, the peritoneum. Antiseptic dressings were applied and one-quarter of a grain of morphine exhibited hypodermatically. The operation occupied about three-quarters of an hour. There was no shock. On the second day tympanites occasioned great distress. The abdomen was tender to touch, but no vomiting had occurred. The temperature was about 100°, the pulse about 100. Opiates were strictly avoided and the patient was placed upon teaspoonful doses of a saturated solution of salts hourly, which, after a few hours, were assisted by enemata of turpentine, glycerin, and soapsuds. This combination, repeated a few times, brought away all of the gas, and in twenty-four hours the belly was flat again and the tenderness gone. From that time, throughout the length of the illness, not the slightest intra-abdominal complication occurred.

The silkworm-gut stitches were removed on the ninth day. The wound was healed, but looked elevated, and the neighboring tissues were somewhat indurated. On the tenth day, the temperature rising to 101.6° and the

wound area feeling more indurated, the line of incision was broken up to the extent of an inch and a pus-pocket exposed. The tip of the little finger introduced into the wound passed through the peritoneum, but nature had evidently walled up the peritoneal side of the incision, ether by lymph or by a fold of omentum, for no intra-peritoneal trouble was present nor did any develop subsequently. On the next morning the temperature and pulse had dropped to normal. In a few days, the temperature rising again, under the belief that the peritoneal silk suture was the sole cause of the mischief, I had chloroform administered, and broke open the wound in its entire extent in search of the suture. A portion of it was found, partially loosened, and caught with forceps. Considerable traction failing to deliver it, and lymph and other inflammatory matters so obscuring the view, it was deemed wiser to keep the wound open and await developments, inasmuch as there was great danger of breaking through the peritoneum and lymph-wall, the extreme thinness of which was very evident to the sense of touch. After several days of waiting, the temperature suddenly mounted to 103.6°, and the pulse to 116. The patient was again anesthetized and the offending suture, considerably looser than before, was removed entirely. The wound smelled foul and was packed with a mixture of iodoform and boric acid after irrigation with mercuric chloride solution (1 : 500), and left open. Mercuric chloride, gr. $\frac{1}{10}$, was administered every four hours, together with a nutrient tonic.

For several days the temperature fluctuated between 100° and 102°, and the patient perspired freely whenever she fell asleep. This perspiration was looked upon as an effort on the part of nature to eliminate a poison, and was not interfered with. No chill occurred during the entire illness. The appetite remained excellent; the tongue moist; the disposition cheerful, and sleep refreshing. The condition gradually improved.

About three weeks after the operation the temperature ranged considerably higher every other day, and this feature became the regular order of affairs. A malarial element seemed present; but the patient had never had any form of malarial fever, although she had for years resided in a swampy country.

Treves, in his recent work[1], writing about the relation between surgical operations and malaria states that, "In a patient who has never had intermittent fever—but who has formerly resided in a malarial district—the wound may apparently give rise to intermittent fever or to intermittent complications," on the theory that "the injury has merely provoked the explosion of a hitherto latent disease."

This seems to have been the case with my patient. At any rate, large doses of quinine had a marked effect upon the character of the manifestations.

At about this time the patient's urine was noticed to have a very strong odor and to be muddy in appearance. Analysis showed the urine to be of a neutral reaction (not alkaline), and to contain triple phosphates in enormous quantity, scattered pus-cells and large numbers of bacteria. The patient was put upon ammonium benzoate (15 grains every four hours), which was gradually diminished in quantity as the urine cleared up and became acid. This it speedily did, urinalysis, in a few days, showing no crystals of triple phosphates. Pus, however, continued to be present in small quantities, occasionally being sufficient to afford a slight trace of albumin.

The last urinalysis, on March 11, 1892, showed still a few pus-corpuscles and scattered epithelial cells from the bladder and ureter.

One other feature of interest is worthy of record. Late in the progress of the case the temperature again suddenly ascended, causing considerable consternation. The cause was finally located in the iodoform mixture which, after non-use for a couple of weeks, had been spread over the granulations on the surface of the wound and had undergone absorption. Bearing upon this point, I quote from Brunton:[1] "In man it (iodoform) may be absorbed from wounds and affect the nervous system, but instead of producing sleep or anesthesia, as in dogs, it usually causes, in slight cases of poisoning, sleeplessness, headache, irritability, and loss of memory."

My patient complained of a bad taste, palpitation of the heart, headache, mental sluggishness, and sleeplessness; but in her case there was a very decided rise of temperature. Brunton does not refer to this latter manifestation; but Shurly, of Detroit, states that in his iodine injections for tuberculosis there is always a marked thermal rise.

The iodoform was withdrawn and the wound again dressed with plain boric acid, and in twelve hours the temperature had fallen to 99°.

The patient sat up on the twenty-eighth day. In a few days, she was wheeled about the corridor in a convalescent chair, and was soon walking unaided about the hospital. She left the city for her home on March 12th, six weeks after the date of arrival.

As to the status of the other kidney, inasmuch as multiple nephritic cystoma is very likely to be bilateral, the chances are against the maintenance of its integrity. At the time of the patient's departure the organ could be felt and easily handled, and seemed to be in fair condition. The same amount of urine was passed as before operation, proving the complete demoralization of the diseased kidney.

A mineral water was recommended for use at home, and urinalyses made since the departure of the patient have shown a continued improvement so far as the urine is concerned. Otherwise the patient is doing exceedingly well, gaining in strength daily, and being occupied more and more in her household duties. In a recent letter she states that she feels splendidly.

In conclusion, I desire to record four practical lessons that this case teaches:

First. The efficacy and value of salts and turpentine in the treatment—particularly the early treatment—of traumatic peritonitis.

Second. The danger of peritoneal suturing as such. (Use silkworm-gut and pass it through the entire thickness of the abdominal wall.)

Third. The possible revival of ancient or lateral paludism in your operations.

Fourth. The susceptibility of patients to the action of iodoform when spread upon a granulating wound-surface.

[1] Manual of Operative Surgery, vol. i, p. 16.

[1] Pharmacology, Therapeutics, and Materia Medica, 3d edition, p. 805.

FAILURE OF DIURETIN TO AVERT URETHRAL FEVER.[1]

BY EDWARD L. KEYES, M.D.,
OF NEW YORK.

I FEEL it incumbent upon me to present a short note in retraction of the claims, or rather pretensions, of diuretin as a drug capable of averting urinary or urethral fever.

These pretensions I put forward modestly at the last meeting of the Society, in Washington, September, 1891, impelled by an unusually successful consecutive series of cases in which, during a number of months, this drug had been freely used and in which there had been no urethral fever.[2]

I claimed nothing except the coincidence of result in a number of consecutive cases, and I promised to investigate further and to report at this meeting.

My report is simple; I need not delay you by detailing cases. In a sufficient number to break all claim for reliability I have administered diuretin freely—and used other precautions—and yet commonplace urinary fever has come on and run the usual course. I am still studying the matter, as we all are, and I trust that sooner or later an infallible prophylactic may be discovered. I have believed that I possessed it several times in my life, but as many times have I been disappointed.

One of the most thorough contributions of recent date upon this subject, in a scientific way, that I am acquainted with is from the pen of Dr. Noël Hallé, in the *Annales des Maladies des Organes Genito-urinaires* for February, 1892, which embodies the present status of conviction of Professor Guyon and his following and the results of the laboratory investigations at the Hospital Necker with the personal work of Hallé and Albarran and citations of recent authorities.

It ascribes all forms of urinary fever—"l'infection urinaire," as they term it—from the simplest rigor to the most pernicious deadly attack, to various but different bacterial infections.

It is needless to say that the subject is not yet cleared up, even by this painstaking study, and the practical side of prevention is hardly touched upon. By such studies light must eventually come, but the way is long and still dark; much mystery surrounds it.

I think I can hardly better illustrate the difficulties attending an attempt at generalization upon this intricate subject than by a detail of one case that illustrates enough well-known features of this peculiar malady to justify its brief reproduction:

A healthy gentleman, thirty-eight years old, with sound kidneys and clear urine, save an occasional urethral shred, consulted me in December, 1891. He had moderate obstructive urethral symptoms, with much suprapubic pain. No. 19 Fr. found two moderate constrictions in the pendulous urethra at the usual sites, but would not pass the bulbo-membranous junction. Several smaller sizes were successively used, but no instrument engaged, so that the attempt to enter the bladder was given up for the day, as the patient's symptoms were not urgent and as no preparation against urinary fever had been made. There was a fair show of blood from the soft deep stricture and a little subsequent hemorrhage, yet all the blood came from in front of the bulbo-membranous junction, beyond which nothing had penetrated. Without other precautions than a little alkaline drink this patient went at once to his home in a neighboring city. He lost some blood and had considerable pain on urinating, but no chill whatever and no elevation of temperature, and his stream became larger and the supra-pubic pains less.

This case illustrates what we so very often see—*i. e.*, a lesion of the mucous membrane drawing blood and some moderate distention of strictures anterior to the triangular ligament, yet no subsequent chill or general reaction, even when no precautions are taken.

This patient now took wintergreen oil freely, continued his alkaline medicine, and filled himself up with quinine. His urine was perfectly clear. In this condition I passed a filiform soft bougie (No. 3 Fr.), in my office, fairly into his bladder. It was slightly grasped on withdrawal, but *brought no blood*. Ten hours later, after reaching home, a few hours after his first urination, he had a violent chill and a temperature of 103°, and within the next forty-eight hours he had five more severe chills, showers of uric-acid crystals and urates, scanty urine containing albumin and casts, and great physical and mental depression. Here, surely, was a good subject for investigation. I gave this patient salol and diuretin freely (gr. xxx shortly before the instruments were again used), and, having him warm in bed, I passed successively No. 3 and No. 6 Fr. soft bougies, bringing a little blood. There was no chill and no reaction whatever, and this was not because he was warm in bed and kept there, as the subsequent history shows.

At the next sitting, under exactly the same conditions, I passed No. 11 French soft bougie. It was very tight; some blood followed, but there was no chill. This was an increase of five sizes French.

At the next sitting I used the same precautions and raised four sizes, to No. 15 French. There was a little more blood than before on the withdrawal of the instrument, but it was slight. All the precautions had been used, yet six chills followed; albumin reappeared and considerable straining on urination and evidences of posterior traumatic urethritis, so that three weeks were allowed to pass before another attempt at dilatation was made. Then No. 15 French was again passed, with all the previous precautions and an extra one.

I recalled Hartmann's statement, made in an article entitled "Fièvre Urineuse,"[1] which I had just read, to the effect that urinary chill was due to the forcible penetration, under a strong urinary effort, of morbid organisms, which were thus driven into the circulation through a newly abraded surface; and I coupled this remembrance with a statement made before this Society by Palmer, of Louisville, that he prevented chill after deep internal urethrotomy by filling the bladder with hot bichloride solution and letting the patient void it after the operation. So I gave the salol and diuretin and after washing the urethra, filled the bladder with a hot bichloride of mercury solution (1 : 3000). Then I passed the No. 15 F. soft bougie, as before. Blood followed, as usual. The patient immediately evacuated, spontane-

[1] Read before the Society of Andrology and Syphilology, June 22, 1892, at Richfield Springs.

[2] THE MEDICAL NEWS, October 31, 1891, p. 505.

[1] Annales des Maladies des Organes Genito-urinaires, Jan. 1892.

ously, his bladderful of hot bichloride solution, and he had no chill and no reaction whatever. I repeated this several times until I reached No. 26 French—the patient's full size, in my opinion—and never a chill or any disturbance followed.

Was it, finally, the bichloride that did the work? I cannot tell, for at last I passed the full-sized instrument without using any precautions. I still brought blood, but no chill followed. My patient was at last acclimated to his bacteria, I suppose. Now he passes his instrument himself, without drawing blood and with no chill.

I have used the bichloride many times since, alone and combined with other means, yet sometimes urinary chill follows. Therefore, I frankly say to-day that I know of no positively certain means of averting urinary chill in every case, but of those I rely on I consider the combination of salol, diuretin, bichloride irrigation, and warmth in bed the best.

109 E. 34TH STREET.

DEATH DURING THE FIRST STAGE OF CHLOROFORM-NARCOSIS.

BY F. W. THOMAS, M.D.,
OF PHILADELPHIA.

THE following report of a case of death under chloroform is presented, because, at the time of its occurrence, its imperfect publication in the newspapers elicited a certain degree of interest, and also for its statistical value in emphasizing the danger of chloroform as an anesthetic.

On July 12, 1892, Mason Boggs, an Englishman by birth, and a "burner-off" by trade, applied to have his left shoulder set.

His appearance was that of a young, well-built laborer.

Three weeks previously the same shoulder had been dislocated, and, according to the patient's statement, the physicians in attendance had "some considerable trouble" in reducing the dislocation. The character of the trouble was not asked. His pulse was strong and regular, slightly quickened, and his respirations were normal. Having once taken ether, he objected to it on the ground that it made him sick. After an unsuccessful attempt to set the shoulder without anesthesia, I determined to administer chloroform to the point of slight relaxation.

A few drops were poured on a handkerchief and held two inches from his nostrils, when immediately a choking sensation, like that produced by ether, was induced. The handkerchief was then withdrawn. After a few moments a second attempt was made, when the patient went into a convulsion and became cyanotic. The anesthetic was withdrawn at once, and artificial respiration and stimulants were resorted to. At first the man appeared to react, his color was restored, and his pulse became full. He then took several deep and regular respirations, turned on his back, and died.

In this case no actual narcosis was produced, and the quantity of the anesthetic used was extremely small. By actual measurement only one and one-half drams of chloroform had been used, of which a large proportion had evaporated while the handkerchief was removed from the nostrils of the patient.

The autopsy, made by Dr. Sidebotham, and which I was permitted to attend by the courtesy of the Coroner, revealed the following condition:

The lungs were normal. The heart was dilated, and its muscular walls hypertrophied and softened. The liver was slightly hardened. The spleen was very large and soft. The stomach contained no food, but was filled with catarrhal mucus, and the mucous membrane was much injected and swollen. The kidneys were enlarged, the left in the greater degree; the capsules were easily removed, and the conective tissue was increased. On section, the kidneys presented the appearance described by the late Dr. Formad as the "cyanotic" kidney. From the employer of the patient I subsequently learned that the man was of very dissipated habits, and especially so in an "alcoholic" direction.

The apparent vigor of the man, the small quantity of chloroform consumed, and the early appearance and fatality of the convulsive stage of narcosis unfortunately emphasize the statement made by Hare,[1] that "In atheletes and drunkards there is a first stage of chloroform-narcosis, characterized by struggling, which it is dangerous to overcome with the anesthetic."

6 MT. AIRY AVENUE, GERMANTOWN.

MEDICAL PROGRESS.

The Influence of Chloroform upon the Course of Normal Labor.—As a result of studies with the tocodynamometer, DOENHOFF (*Archiv für Gynäkol.*, Bd. xlii, H. 2, p. 305) has found that chloroform when given to a degree productive of slight narcosis (analgesia) exerts a paralyzant influence upon uterine contraction. If the narcosis be continued, the sum-total of the force of the contractions progressively diminishes. In slight narcosis the activity of the contractions, as well as the duration of the intermissions and the strength of the pains, becomes irregular. In profound narcosis the intermissions are equally long, and the pains equally feeble. At the conclusion of the narcosis the pains again become more active. The sum-total of the force of the pains immediately after narcosis bears a relation of 2 to 3 to that preceding the narcosis. After narcosis the activity of the pains remains for a considerable time slighter than before narcosis. If previously exerted moderately, the action of the abdominal muscles ceases early during slight narcosis. Soon after narcosis it returns with corresponding force. During the narcosis the abdominal muscles are inert; even after the period of dilatation, if abdominal contractions have been active, they continue after partial narcosis, but with less frequency and in less degree; they cease entirely during profound narcosis. The intermissions become longer early in slight narcosis, and the number of pains smaller.

Pyloroplasty for Cicatricial Stricture.—LIMONT and PAGE (*Lancet*, No. 3593, p. 84) have reported the case of a man, thirty-one years old, who, for fourteen years, following a severe blow in the epigastric region, complained of recurrent attacks of vomiting, dragging pain in the abdomen, loss of flesh, and constipation. The stomach was greatly dilated, and a hard nodule, as large as the distal phalanx of the little finger, could be felt in a situation corresponding with the pyloric extremity of the

[1] Therapeutics, p. 126.

stomach. A diagnosis of traumatic stricture of the pylorus was made, and, after the unsuccessful employment of lavage, operation was determined upon. After the stomach was irrigated with a solution of boric acid, a median incision was made from the ensiform cartilage to the umbilicus. Cicatricial narrowing of the pylorus was found. The cicatrix was excised. The mucous and peritoneal edges of the wound were then sewed together over the muscular coat with a continuous suture of fine silk. The left angle of the wound (terminating in the stomach) was secured to the right angle (terminating in the duodenum) with a Lembert suture of silk. The wound was closed by a double row of Lembert sutures and covered with a graft of omentum. For eight days the patient was fed by the rectum. Subsequently, he took food by the mouth, gradually returning to his ordinary diet. He rapidly regained weight, and was soon able to resume his usual occupation.

Anesthesia.—At the recent meeting of the British Medical Association, DR. LAWRENCE TURNBULL, of Philadelphia, reported that he had gathered statistics of 1898 cases in which anesthetics had been employed. Of this number ether had been used in 1500; chloroform in 298; the A. C. E. mixture in 100. There were 5 deaths during or shortly after anesthetization: 1 from chloroform; 3 from ether; 1 from the A. C. E. mixture. Dangerous symptoms appeared in 12 cases: in 2 from ether; in 9 from chloroform; in 1 from the A. C. E. mixture. Opinion was divided as to the best anesthetic; the majority considered ether, chloroform, and the A. C. E. mixture equally safe. It was agreed that the best method and manner of administering anesthetics is by the cone, various inhalers, and napkins. Ether is to be preferred for weak subjects; chloroform for strong subjects or when disease of brain or kidney exists. The most effective methods of restoring patients threatened with death from anesthetics include inversion, placing the head lower than the body, and administering stimulants hypodermatically: alcohol and atropine, and strychnine. The smallest number of students are taught practically how to administer anesthetics; the remainder are taught theoretically.

Craniotomy for Double Optic Neuritis with Microcephalus.—MILLER (*British Medical Journal*, No. 1647, p. 176) has reported the case of a male infant, eight months old, presenting microcephalus and double optic neuritis. The spine and extremities were almost constantly extended and rigid; the thumbs were adducted and the fingers flexed; the legs were also adducted and crossed. The genitalia were imperfectly developed. There were nystagmus and internal strabismus. The eyeballs were deeply drawn into the orbits. The facial bones were well developed; the lower central incisors were well through the gums; all of the cranial sutures were completely ossified, and the fontanels were obliterated. Delivery had been accomplished with the aid of forceps. The bony eminences of the cranium were but slightly marked; the forehead was receding and narrow, and the circumference of the head was under the normal. The child was strongly built, but restless; it cried a great deal and was habitually constipated. It displayed little intellectual activity. Linear craniotomy was performed, a half-inch of bone being removed throughout an extent of three inches. The improvement was immediate, decided, and progressive.

THERAPEUTIC NOTES.

Potassium Tellurate for the Hyperidrosis of Tuberculosis.—POHORSKI (*Wiener medizin. Presse*, No. 32, p. 1293) reports the employment of potassium tellurate in fifty cases of tuberculosis for the relief of excessive sweating. The remedy was given in doses of from gr. ¼ to gr. j daily. In marked cases the sweating ceased after evening doses of from gr. ¼ to gr. ss. As a rule, larger doses were required to control the sweating that occurred at night than were required to control the sweating that occurred in the morning or in the course of the day. The good effects of the remedy became apparent a quarter of an hour after ingestion and continued in obstinate cases for from five to seven hours; in mild cases the effect was more protracted. In many cases a tolerance was established, so that it became necessary to increase the dose. Only in exceptional cases were untoward effects upon the digestion observed, which subsided on the withdrawal of the remedy.

For Tonsillitis.—The following prescription is recommended for use as a prophylactic for persons predisposed to the development of tonsillitis:

℞.—Olei menthæ piper. . . . ♏viij.
Acid. carbolic. crystal. . . . ʒj.
Spts. vini rectif. . . . . ʒij.—M.

S.—Ten drops in a cup of warm water, morning and evening, as a gargle.—*Correspondenzbl. f. Schweiz. Aerzte.*

Menthol for Hyperemesis Gravidarum.—WEIL (*Centralbl. f. die gesammte Therapie*, J. x, H. viii, p. 449) reports the successful employment of menthol in the treatment of a case of hyperemesis gravidarum. The drug is best given in oil, of which ten drops of a twenty per cent. solution are placed upon sugar in a spoon and surrounded by more sugar, the dose to be repeated whenever nausea appears.

A Tonic Laxative.—

℞.—Aloin . . . . . gr. ij–iij.
Tinct. nucis vomicæ, Tinct. belladonnæ, Tinct. physostigmatis . . āā ʒij.
Ext. cascaræ fluid. . . ʒvi.—M.

S.—Take thirty drops three times daily, reducing the dose one drop daily.

For Acute Bronchitis.—

℞.—Tinct. sanguinariæ, Tinct. lobeliæ . . . āā ʒj.
Vini ipecac. . . . . ʒij.
Syrup. tolutan. . . . . ʒiv.—M.

S.—A teaspoonful every three hours.

BARTHOLOW.

For Myalgia.—

℞.—Ext. cimicifugæ fluid., Ext. erythroxyli fluid., Tinct. guaiac. ammon. . . āā ʒj.—M.

S.—Take a teaspoonful three times a day.

THE MEDICAL NEWS.

A WEEKLY JOURNAL

OF MEDICAL SCIENCE.

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SATURDAY, SEPTEMBER 24, 1892.

THE PROTOZOA IN HUMAN PATHOLOGY.

AFTER long and exclusive devotion to the study of vegetable organisms it is refreshing to have attention directed to the low forms of animal life known as protozoa as causes of disease; and especially is this satisfactory as such investigation holds out the promise of clearing up the hitherto obscure nature of some of the commonest of human affections.

The favorite ancient theory taught that all diseases depend upon minute animal organisms, so that attention was ever directed to the discovery of such animalculæ. The importance of the large number of vegetable forms has for the time diverted attention from the part played by animal organisms in the production of disease. This has been further contributed to by the fact that few medical men have brought to the study that knowledge of the lower branches of zoölogic science which is requisite for the discovery and description of the protozoan parasites, and among pure zoölogists few have been attracted to this study at all. Furthermore, we have lacked, and still want, methods of cultivation and isolation, such as have made the description, classification, and complete study of bacteria easily possible.

In consequence there are few, if any, of the protozoa of which we can claim to have more than fragmentary knowledge. The awakening has, however, come. General attention is aroused, studies that will facilitate further observations have been made, and even cultivation to several generations has been attained. We may, therefore, hope for the establishment of methods of research that will make it possible to learn the nature and properties of the forms already known, and doubtless also to discover forms at present unknown.

The relation of protozoan forms to malarial fever and to dysentery, and the promise of a solution of the etiology of carcinoma, have added practical interest to a study that heretofore seemed of merely scientific value; and whatever may be achieved in the future will in a large measure be ascribable to the interest aroused by these observations.

Of the three classes of *protozoa—sarcodina*, *sporozoa*, and *infusoria*—it is noticeable that the most conspicuous, and therefore the earliest and best known, the *infusoria*, are apparently of least importance in human pathology. Everyone who has microscopically studied the excretions or secretions of the body must frequently have seen the conspicuous Flagellata; and when the observations have been made in case of disease and the organisms were abundant, the question of the etiologic relationship must have arisen. It is not unnatural, therefore, that such forms as *Trichomonas intestinalis*, occurring abundantly in the stools in various kinds of diarrhea, have been considered as the cause of diarrhea, of cholera, of typhoid fever, and other diseases. So closely does the intensity of the disease sometimes seem to be related to the number of organisms that, as in the case of ORTMANN (*Berliner klin. Wochenschrift*, August 17, 1891), one is almost forced to the conclusion that by their numbers and activity these organisms may be capable of exciting inflammatory action, if not specific effects. The weight of authority (LEUCKART, CUNNINGHAM, PFEIFFER) is, however, disposed to acquit the infusoria of any participation in the causation of disease, and to explain their presence as saprophytes.

The *sporozoa*, on the other hand, are of immensely greater importance in human medicine, and include a variety of organisms bearing the closest relation to internal and external diseases. It is needless, at this place, to enter into any discussion as to the exact biologic position of the "plasmodium" of malaria, for this could but lead to a general discussion of the life-history of the different protozoan

forms, and the discovery of nothing but the great uncertainty of any classification yet offered.

The malarial organism, as indeed all of the *hematozoa*, bears a striking resemblance to the *sporozoa*, and may, therefore, be considered as one of them. We have still much to learn regarding this organism, regarding the relationship of the different forms seen in tertian, quartan, and irregular ague, and regarding its relationship to the parasites of the blood of birds, frogs, turtles, and other animals. As to its bearing on malarial fever, it may be truthfully said that "not a single observer who has had the necessary training and the material at his command has failed to demonstrate the existence of these parasites" (OSLER); and though it cannot be said that a conclusive demonstration of the causative relationship of the malarial organism has been offered as in the case of bacteria, there is the strongest presumptive evidence for such an assumption.

The detection of the organism as a diagnostic factor has been of extreme value in many instances, and it has served to elucidate the nature of certain cases of fever occurring in irregular paroxysms at intervals of five, six, eight, ten, or twelve days or more. The observation of hematozoa closely allied to the malarial organism in the blood of frogs, turtles, birds, and other animals may lead to a completer knowledge of the life-history of these organisms; and the discovery by DANILEWSKY of acute malarial paroxysms in birds, attended by the presence in the blood of organisms scarcely distinguishable from those found in the blood of human subjects having malaria, adds another link to the chain of evidence in favor of the pathogenicity of the animalcule.

A group of *sporozoa* better and longer known than those just mentioned are those coccidia or psorospermia first observed in the liver and gall-ducts of rabbits, and subsequently also in the intestinal tract of rabbits, mice, and other animals, and finally of man. The peculiar nature of the growths in the liver of rabbits, in association with which psorospermia have been so constantly found, leaves no doubt of the actual causative relation of the organisms to the disease. The great difficulty in the recognition of protozoan organisms is well illustrated by their having been taken, by competent observers, for eggs of *distoma*, though at present such a mistake would no longer be excusable. Macroscopically, however, the resemblance of the nodules in the liver to tuberculous new-growths is not a little marked, as was pointed out by SILCOCK in the description of his case; and it is quite possible that this parasitic invasion may be far commoner than is yet believed or known.

The discovery of psorospermiasis of the liver in man by GUBLER, HADDON, SILCOCK, and others; in the ureters by BLAND SUTTON and TARGETT, is sufficient evidence that the subject is deserving of closer attention than it has hitherto been accorded; for though, from the known life-history (the fact that they do not pass through their various phases in the same host, and therefore repeated ingestion is necessary to bring about pathologic changes) of organisms of this class, it is unlikely that psorospermial invasion is anywhere frequent, it is probable that the disease is more common than is believed.

Of far greater importance is the rôle of coccidia in external diseases, especially various affections of the skin. There seems no longer any doubt that organisms of this class occur in *molluscum contagiosum*, in the *keratosis follicularis* of WHITE or *psorospermose folliculaire végétante* of DARIER, and in *Paget's disease* of the nipples; and the relation of the organism to the extension of the pathologic processes and to the individual cells would indicate a more than accidental relationship. From the papillomatous excrescences of the gall-ducts in the liver of the rabbit or of man to the new-growth in molluscum contagiosum, to Paget's disease, and, finally, to epithelioma and carcinoma, is a sequence of pathologic processes of considerable similarity, and it need not, therefore, occasion surprise that carcinoma likewise is ascribed to psorospermial activity. The magnitude of this assumption is such that convincing proof must be forthcoming before its demonstration can be admitted.

As long as forty years ago VIRCHOW detected certain hyaline bodies within the epithelial cells in carcinoma, but ascribed these formations to hyaline degeneration. Subsequently little attention was paid to these appearances, and rarely was more than passing notice given them until a few years ago, when English observers noted their resemblance to the intra-cellular parasites of lower animals, and suggested their protozoan nature. It cannot any longer be denied that these structures have a distinct existence, and are in no sense products of technique, and the only point at issue is whether they are parasitic or degenerated protoplasm.

In this connection there will, perhaps, be the

same skepticism as attended and followed LAVERAN'S demonstration of the malarial organism, and as BUETSCHLI pronounced in favor of the parasitic nature of this organism, so we have the authority of METSCHNIKOFF in favor of the parasite in carcinoma —authority that on this point is greater than that of VIRCHOW, who still maintains the hyaloid nature of these formations. In the absence of any well-defined structure and of methods of propagation it is, of course, exceedingly difficult to decide the question without frequently repeated observations, but opinion is strongly inclined to view these bodies as protozoan. The frequency with which they have been found in the various forms of carcinoma by different observers (SIMS-WOODHEAD, RUSSEL, RUFFER and WALKER, PODWYSSOZKI and SAWTSCHENKO, and many others), and their absence from other and similar new-growths are sufficient to excite interest, even if no conclusions can be drawn from the facts observed.

Enough has been said to indicate the immense importance of further research along the lines laid down. What is now most needed is some method by which the organisms, if such they be, may be cultivated and studied in their several phases.

Of the last class of protozoa, the *sarcodina*, the group Rhizopoda alone merit consideration here, and of these mainly the ameban organism discovered and described by LÖSCH as the cause of dysentery. This subject has already been fully considered (THE MEDICAL NEWS, July 25, 1891, p. 107; January 16, 1892, p. 79), and for the present we need only point out again the difficulty of classifying these low forms of animal life; for while some writers are content to regard this as a permanent ameban form, there are others (CUNNINGHAM) who look upon it as a phase in the life of an organism that at another period is seen as a monadine, and, like other *infusoria*, is only of secondary importance (a result rather than a cause) in the disease in which it occurs.

Instances of the relation of protozoan parasites to human disease need not be further multiplied. Sufficient has been said to show that we are approaching a fuller knowledge of certain obscure problems in pathology, and though we cannot believe with PFEIFFER that the study of these organisms will grow into a special science, as has bacteriology, we must admit the necessity for further research and the rich promise that it holds forth.

PNEUMATHEMIA.

THIS name was coined by CLESS to denote a condition in which air is present in the bloodvessels.

In the medical literature of the past seventy years, many cases are recorded in which the bloodvessels after death contained air or gas, which did not seem attributable to post-mortem decomposition. In some cases, especially in the course of surgical operations about the neck, death has been supposed to result from the entrance of air into the veins. Such cases have been the subject of much dispute.

Experimental work has been done that proves that very large quantities of air must enter the vessels for death to ensue; thus NYSTEN found the lethal amount introduced into the jugular veins of dogs to vary between 40 and 120 c.c. HARE also proved that quantities of less than 40 c.c. did not kill dogs, and expressed his belief that an amount of air sufficient to cause death in a human being could not possibly enter a vein injured by a surgeon during an operation. LABORDE and MURON have further shown that enormous quantities, 2000 or 3000 c.c., if introduced slowly and at intervals into the circulation of dogs, do not cause death.

While not denying that death really may at times result from the sudden entrance of air into injured veins, WELCH and NUTTALL ("A Gas-producing Bacillus—*Bacillus aerogenes capsulatus, nov. spec.*—Capable of Rapid Development in the Bloodvessels after Death," *Johns Hopkins Hospital Bulletin*, 1892, No. 24) have reported some important investigations that will serve to clear up many, if not all, of these mysterious cases.

In the case of a man who died rather suddenly after repeated copious hemorrhages from a large sacculated aneurism of the aorta, which had ruptured through the anterior thoracic walls in two places, the heart and bloodvessels eight hours after death, were everywhere found to contain gas-bubbles in large amounts. The gas was inflammable, and was, therefore, certainly not atmospheric air. Smear cover-glass preparations of the blood showed myriads of capsulated bacilli wherever this gas was found. The microörganism was isolated in pure culture and found to be an obligatory anaërobe. The bacillus, which varies in size, averaging from 3 to 7 μ in length, is non-motile, does not form spores, is, as a rule, surrounded by a distinct capsule, grows on all ordinary culture-media under anaërobic condi-

tions, and stains well with the usual aniline dyes. Pure cultures in sugar-bouillon, injected into the circulation of rabbits just before death, reproduced in every particular the conditions found in the body of the patient, so far as the presence of gas and the changes caused by the bacillus are concerned. WELCH and NUTTALL express the belief that the bacilli entered the aneurismal sac through the external openings in the chest-wall before death, and, though speaking with reserve on this point, they think that the gas produced may have had something to do with the death of the patient.

We do not know whether cultures have before been made in such cases, but even if they had been made by the ordinary roll and plate methods, it would not be surprising that no results were obtained, since, as has now been discovered, the bacillus is anaërobic. This instance shows the advisability of adding, as a matter of routine, to our ordinary culture-methods at autopsies, a few tubes to be grown in the absence of oxygen, a matter of little difficulty since the introduction of BÜCHNER'S jars.

A CABINET OFFICER OF HEALTH.

IF the wisdom and the necessity of having a National Officer of Health, with a place in the Cabinet of the President of the United States, required demonstration, recent events have amply furnished such demonstration. With a Secretary of Hygiene at the head of a National system of quarantine, the danger of invasion from without by infectious disease would be reduced to an infinitesimal minimum. Such an officer, well qualified, thoroughly equipped, fully authorized, could at once, whenever occasion demanded, institute appropriate measures for the conservation of the public welfare and the protection of the health and lives of the people not only of a city or State, but of the whole country.

The usefulness of such a Cabinet-officer would, of course, not be confined to the exclusion of disease that threatens to enter from without, but would likewise make itself felt in preventing the development of disease and controlling its spread within the borders of our country. By raising to the highest standard the general hygienic condition of the people, the danger of infection of all kinds and from all sources would be greatly diminished, while the power of resistance to disease and the aggregate length of life would be greatly increased, thus saving to the State in the usefulness of its citizens more than any expenditure that could be entailed in the maintenance and administration of the office. It might also be found desirable to place under the control of such an officer the medical direction of the various departments of the Government—the army, navy, marine-hospital service, pension, etc.

A National Health-officer could accomplish a vast deal of good. The question of expenditure, as has been said, could not be raised, because the suppression of a single epidemic of typhoid fever, of scarlet fever, of diphtheria, of smallpox, of yellow fever, or of cholera, with the attendant saving of life and conservation of commerce, would more than compensate for the comparatively small outlay that would be necessary for the administration of the office. At the same time, the profession of medicine, including as it does the arts of sanitation, of hygiene, and of demography, would receive a recognition that, from the important relation that it holds to the welfare of the individual and the community, it deserves to receive. The time is ripe to have a Cabinet Officer of Health.

THE UNITY OF CHOLERA.

TALAMON (*La Médecine Moderne*, 1892, No. 35, p. 543,) adduces numerous arguments in the endeavor to demonstrate the identity of cholera Asiatica and cholera nostras. He points out that epidemics of choleriform diarrhea occur from time to time without evidence of importation, in places in which true cholera has at some period in the past been present. He also refers to the recent epidemic, and to another in 1886, in the neighborhood of Paris, in which clinically the disease was diagnosticated as cholera nostras, but in which bacteriologic investigation demonstrated the presence of comma-bacilli in the intestinal evacuations.

The proposition is a most important one and is worthy of further investigation, in order that its accuracy may be proved or disproved. It would be a most interesting development if it were shown that a disease so deadly as cholera suffers a decided modification of its virulence by changed climatic, meteorologic, and geologic conditions, and perhaps as a result of hygienic and sanitary provisions. The evidence, however, is purely circumstantial, and needs to be substantiated by trustworthy bacteriologic investigation.

It would, indeed, be remarkable if the identity of

cholera nostras and cholera Asiatica had escaped the observation of bacteriologists, more particularly as it has been claimed by FINKLER and PRIOR that they have found in the stools of patients with cholera nostras organisms resembling the comma-bacilli of KOCH.

A corresponding attempt has been made to disprove the specificity of the typhoid bacillus of EBERTH, the identity of which with the bacterium coli commune it has been endeavored to demontrate, thus far, however, we believe without success. In this connection, too, we venture to predict that it will be found that the two forms of cholera, as they have heretofore been recognized, will be found to be distinct.

SELECTION.

THE PLACE OF ANTHROPOLOGY IN MEDICAL EDUCATION.

VIRCHOW, who adds to his other claims to fame that of being the first of living anthropologists, has recently confessed that his attention was directed to the science of anthropology by the difficulties he encountered in the study of the insane. Charcot, again, frequently impresses on his pupils the importance of studying the healthy nude, and of an acquaintance with anthropometric canons, as an aid to the diagnosis of abnormal conditions. These utterances of two of the most honored of our teachers in very different fields suggest that there is a defect in our medical courses which demands, at the least, some consideration. As evidence of the close relationship between anthropology and medical practice, it is enough to mention that in spite of the difficulties we at present place in the way, with a few exceptions (in which zoölogy alone led up to anthropology), the chief anthropologists of the last half-century have been medical men—in not a few cases very distinguished in the profession; at the least, they have started as students of medicine. While every medical man would find a slight acquaintance with anthropology some help in practice, there are certain branches of practice in which some knowledge of anthropology is of especial assistance; for example, practice abroad and asylum practice.

The exact study of madness is making rapid progress; it is beginning to be recognized that the great truth that knowledge means measurement (*scire est mensurare*) fully applies to the brain and nervous system. But in this country the rule-of-thumb method still reigns nearly everywhere.

We are often told that the medical student of to-day is overburdened with study; and, although it must be remembered that the period of his studies is now being enlarged, there is no doubt truth in this statement. It becomes the more necessary, on the one hand, to place in a period antecedent to medical studies proper the preliminary scientific courses; and, on the other hand, to cut away without remorse those branches of knowledge which have ceased to possess any close connection with modern medicine. In certain directions it is probable that the studies of medical students might with advantage be abbreviated or rendered optional. The study of botany, however valuable and fascinating, no longer possesses any special advantage as a preparation for medical practice, now that the physician is very clearly differentiated from the herbalist and "medical botanist." An exact knowledge of the Pharmacopeia also, which once embraced almost the largest part of the doctor's work, may now safely be left to the medical antiquarian. If it is necessary to make room for anthropology by the omission or contraction of other preliminary courses, it is not difficult to put one's finger on studies which for the student of medicine have come to possess a value which is merely traditional.

The time seems to have come when some small preliminary step in the direction here indicated should at length be taken. It is not necessary for the medical man of to-day to know much of the lower animal forms; still less necessary is it that he should have any thorough knowledge of plants. But it is increasingly necessary that he should understand the science of man. — HAVELOCK ELLIS, L.S.A., *in The Lancet, August 13, 1892.*

REVIEWS.

A NEW PRONOUNCING DICTIONARY OF MEDICINE: BEING A VOLUMINOUS AND EXHAUSTIVE HAND-BOOK OF MEDICAL AND SCIENTIFIC TERMINOLOGY, WITH PHONETIC PRONUNCIATION, ACCENTUATION, ETYMOLOGY, ETC. By JOHN M. KEATING, M.D., LL.D., and HENRY HAMILTON, with the collaboration of J. CHALMERS DA COSTA, M.D., and FREDERICK A. PACKARD, M.D. Philadelphia: W. B. Saunders, 1892.

WHILE the late Mr. Henry Hamilton, who was the reviewer's personal friend, was sacrificing his valuable life to this work, he was repeatedly warned not only of this danger, and of the doubtful wisdom of the brightest non-medical mind attempting a distinctly medical task, but also of two more certain and decided errors that he was committing: the non-use of the best works upon medical lexicography, and the complete ignoring of the tremendous number of new word-coinages of the past few years in medical text-books and periodical literature. The best new dictionaries in French and German, the Sydenham, Foster, and *The Century* in English, were not at hand or not utilized, and modern medical text-books and periodical literature were not gleaned.

From the foregoing facts and from others, it gives us positive pain to be compelled by duty to say that the two great mistakes mentioned so far mar the usefulness of the book and belie the title that one wonders at the final permission to publish. We would most gladly have commended the book and its many excellences, but these excellences are neutralized by the fact that the old literature is not included and the new is not known.

A book must be judged by what it pretends to be. Is this "a voluminous and exhaustive hand-book of medical and scientific" words (why does the title say *terminology*)? Alas, far from it! Thus, on the first page: *Abasia* is not given; (it is given as a definition of *astasia*, which it is not). The following are only a few of the

words and terms under the letter *A* unaccountably omitted:

Abirritation, Ablactation, Artificial, embryonic, and inevitable Abortion, The kinds and phenomena of Accommodation, Accrementition, A. C. E. Mixture (not under *mixture* or *A*), many *Acnes, Aërobia* and *Aërobic* (*Facultative Aërobia*), *Anaërobia, Anaërobic, Aërotherapeutics, Aërteriversion, After-images, Albumoses* (several kinds), *Aleze, Allorrhythmia, Alteration Theory, Amphibia, Amphicreatine, Amyencephalus, Amyloses, Anadicrotic, Anakusis, Angeioneurosis, Antithenar, Antilobium, Anther, Apolar, Apollinaris, Apselaphasia, Apus, Arecin, Arthroclasia, Arthroempyesis, Arthrography, Asternal, Asternia, Asymmetry, Athelasmus, Atlodymus, Autoclave, Autopathic, Autophagy, Autosite, Auxocardia* (*Meiocardia* also), etc. Under the letter *D* we find absent *Demography, Delomorphous, Demutization, Denutrition, Deradelphus, Derencephalus, various forms of Dermatitis, Derodymus, Desault, Desmography, Deutoplasm, Devitalize, Diamagnetic, Diaphanoscopy, Diapophysis, Diaster, Diblastic, Dicephalus, Diffraction, Digit, Dilemma, Diphthongia, Diplakusis, Diplococcus, Disparate, Dispareunia, Dispirem, Diver's Paralysis* (*Caisson Disease*, also), *Dochmius, Dolichohieric, Dolichopellic* (*Platyhieric*, also), *Dosimetry, Doyère, Dropped Hand or Wrist, Drum-belly, Dysbasia, Dyschromatopsia, Dystrophy*, etc., etc. The lists would become absurd in length. We add but a few more omissions that in glancing have excited comment: *Pyoktanin, Microphotograph, Toxin, Toxalbumin, Tracheal Tugging, Wallerian Law of Degeneration, Weigert's Method, Virile Reflex, Position of Fetus, Capuron Cardinal Points of, Psychophysics, Karyolysis, Wreath, Rosette, Tuberculocidin, Trendelenburg's Position, Waterhammer Pulse, Ectoplasm, Endoplasm, Endosperm, Eosinophilous, Labarraque's Solution, Polyesthesia, Pithing, Control-animal or Control-experiment, Plate-culture, Chemotaxis, Phocomelus, Hemimelus, Micromelus, Ectromelus, Symelus, Uromelus, Sirenomelus, Proencephalus, Iniencephalus, Exencephalus, Syncephalus, Heterodymus* [one would judge that the authors did not include teratology in medical science!], *Heterogamy, Heterocephalus, Hetero-infection, Epignathus, Hypognathus, Laveran's Organisms, Phenacetin, Creolin, Immunity, Immunization*—but we shall not tire the reader further!

There is some excellent matter in the appendix, but it is rendered utterly useless by the total lack of cross-references from the body of the book. Whatever savors of newness is not even mentioned in the body of the work, as is seen in the Tables of Bacteria, Newer Drugs, etc. To take only two pages as an example: on pp. 796–97, twenty new drugs are mentioned, and not one has a cross-reference, whilst of the twenty, thirteen are not to be found in the dictionary proper.

THE PRINCIPLES AND PRACTICE OF BANDAGING. By GWILYM G. DAVIS, M.D., Member of the Royal College of Surgeons, England; Assistant Demonstrator of Surgery, University of Pennsylvania, etc. Pp. 81. Profusely illustrated. Detroit, Mich.: Geo. S. Davis, 1891.

THE perusal of this volume has given the reviewer genuine pleasure. The author presents his subject in a clear and concise manner under the three divisions: the rolled bandages, the tailed bandages, or slings, and the handkerchief bandages.

In the preface Dr. Davis states that "bandaging, while not a science, is, nevertheless, governed by something more than mere empiricisms, and it is one of the objects of this work to direct attention to the fundamental bandage and to the importance of first learning principles and then their application in the form of the various special bandages." We think that Dr. Davis has more than accomplished the purposes thus set forth, for while bandaging is considered to some extent in the various treatises on general surgery, and more fully in a few published books devoted to minor surgery or to bandaging, the subject of modern bandaging has not previously had the exhaustive attention given it which is displayed in the text of the volume under consideration.

The illustrations are taken from original drawings made by the author, and are numerous and excellent. There are 172 illustrations, which are printed from 23 plates, each plate occupying a separate page. Each illustration is numbered in the text, so that reference to it is easy. The graphic skill shown in the preparation of the sketches conveys with accuracy the lessons of the text and enhances the value of the book.

The book is handsomely printed on good paper with wide margins.

Had the publisher's price ($3.00) been placed at a lower figure, so as more nearly to meet the means of the student's purse, the book would have met with a popular demand as a text-book from this source, for it is certainly the most modern and complete work on bandaging that we have seen.

TRANSACTIONS OF THE THIRTEENTH ANNUAL MEETING OF THE AMERICAN LARYNGOLOGICAL ASSOCIATION. Pp. 165. New York: D. Appleton & Co., 1892.

THIS report of the meeting of the American Laryngological Association held more than a year ago presents a summary of recent investigations in laryngology and rhinology, and the seventeen papers are so uniformly good that it is difficult to make a discrimination.

"The Tonsils in Health and Disease," by Dr. Harrison Allen, is a subject that has awakened considerable interest among the members of the profession, and the article may be read with profit by everyone, although after all we are not informed what is the *function* of the tonsils. The paper that has perhaps been commented upon most widely is that by Dr. Bosworth, in which he discusses nasal disease as a factor in the production of asthma, and the excellent and permanent results obtained by simply removing intra-nasal pressure. Others have also reported similar success with the same treatment. Dr. Delavan gives a very interesting account of the surgical methods employed in the treatment of tuberculous laryngitis. Dr. Robinson deals with the relations of disturbances of the upper air-passages to constitutional diseases. These and other papers of almost equal interest, and by eminent specialists, combine to make the book of value to the profession and constitute a worthy contribution to the literature of the subjects with which they deal.

TEMPERAMENT, DISEASE, AND HEALTH. By FRENCH ENSOR CHADWICK, Commander, U. S. N. New York and London: G. P. Putnam's Sons, 1892.

THIS small effusion of eighty-five pages is a curious rehash of well-known facts and theories, and, while rambling in style and rather indefinite as to its object, contains much that is suggestive. In the first chapter the author discusses the proposition that differences in temperament result from the varying rates of molecular change in different individuals. This seems self-evident when one considers the subject carefully, and yet our author certainly formulates his ideas in such a manner that the amount of influence exerted by this difference in molecular metamorphosis is impressed upon the reader more forcibly than would be the case without some such arguments as are here brought forward.

The second chapter is devoted to a short *résumé* of the facts now known in bacteriology. While it would seem that an undue share of this small volume is devoted to a subject rather remote from that indicated by the title, it is, nevertheless, a well-written and concise account of our knowledge upon the subject of the relations of microörganisms to disease.

The whole substance of the book may be summed up in the following words of the author: "A perfect and normal working of the processes of elimination thus becomes the one sole and vital basis of what we understand by physical health."

SOCIETY PROCEEDINGS.

MEDICAL SOCIETY OF VIRGINIA.

Twenty-third Annual Session, held at Alleghany Springs, Va., Sept. 13–15, 1892.

FIRST DAY—SEPTEMBER 13TH.

AMONG the invited guests and fraternal delegates present were Dr. J. A. Tanner, of Boston; Dr. J. Price and Dr. G. Betton Massey, of Philadelphia; Dr. S. T. Earle and Dr. Wm. Lee Howard, of Baltimore; Dr. Samuel J. Fort, of Ellicott City, Md.; Dr. Irving C. Rosse, of Washington, D. C.; Dr. Edw. Ricketts, of Cincinnati; Dr. Pratt, of New Orleans. Beside these, there were in attendance about 160 members of the Society from all parts of the State.

During the session Dr. Joseph Price, of Philadelphia, Dr. H. Grey Latham, and Dr. Landon B. Edwards were elected Honorary Fellows of the Society.

The officers elected for the ensuing term are:

President: Dr. Herbert M. Nash, of Norfolk.

Vice-Presidents: Dr. Wm. P. McGuire, of Winchester; Dr. T. James Taylor, of Walthall's Store; and Dr. A. Z. Koiner, of Roanoke.

Recording Secretary: Dr. Landon B. Edwards, of Richmond.

Corresponding Secretary: Dr. J. F. Winn, of Richmond.

Treasurer: Dr. Richard T. Styll, of Hollins, Va.

Chairman of the Executive Committee: Dr. Hunter McGuire, of Richmond.

Chairman of the Publishing Committee: Dr. Hugh M. Taylor, of Richmond.

Chairman of the Committee on Applications for Fellowship: Dr. Wm. D. Turner, of Fergusson's Wharf.

Chairman of the Necrological Committee: Dr. John S. Apperson, of Marion.

To deliver, in 1893, the Annual Address to the Public and Profession: Dr. J. T. Graham, of Wytheville. The subject for general discussion during the session of 1893 is "Chronic Nephritis," in which Dr. R. M. Slaughter, of Theological Seminary, Va., is to be the leader. The place of the next Annual Meeting is Charlottesville, Va., at such a time during October, 1893, as the local profession may select.

The State Board of Medical Examiners for the quadrennial term, to begin January 1, 1893, is to be composed of about the same gentlemen as are now serving on it.

DR. JACOB MICHAUX, of Richmond, delivered the Annual Address to the Public and Profession, announcing as his subject, "The Medical Examining Board." He stated that the only means of reforming medical education was by such a Board. The main points of his address were the absolute necessity for: 1. Preliminary academic examinations before admission to the medical schools is allowed; and 2. Separation of the teaching and examining departments. He suggested that the various State Examining Boards be made Boards of Health. He cited the much higher requirements of the European countries in both medical and academic branches.

The Hunter McGuire Prize of One Hundred Dollars for the best essay on "Tetanus" by a member of the Society was awarded to Dr. Chas. M. Blackford, of Lynchburg, Va.

The results of the investigations were summed up as follows:

1. Tetanus is essentially a tonic spasm of certain muscles which have been thrown into a state of physiologic contraction.

2. Abnormal irritability of the reflex centers in the medulla and cord are caused by it.

3. The physiologic action of certain ptomaïnes, or alkaloids of decomposition formed in the wound, and absorbed therefrom, are the results of this hyperesthesia.

4. These ptomaïnes result from the growth of a specific bacterium called the bacillus of tetanus or the bacillus of Nicolaier, and only from it. Therefore,

5. Tetanus is a toxic disease caused by the infection of a wound by this specific bacterium or its products.

In treatment he recommends that the wound be at once explored and cleaned of all foreign substances with scrupulous care. The recesses of the wound should be opened and a germicide applied. Because of their undoubted power to destroy the ptomaïnes—perhaps through the iodine they contain—iodoform or one of its congeners should be freely used. The chief aids in rendering the wounds thoroughly clean, and which should be used first, are corrosive sublimate and carbolic acid—the iodoform or other iodine combination to be used after disinfection. If tetanus occurs after thorough asepsis and antisepsis, then the wound has been contaminated by some error of omission or commission on the part of the surgeon. The current idea that wounds of the hands and feet are peculiarly liable to be followed by tetanus, is due to the fact that it is more difficult to cleanse them and they are more liable to get dirty.

The first indication is to check the development of the

ptomaïne, should the attack have commenced. To do this the wound should be opened and disinfected, if necessary with the actual cautery. Corrosive sublimate (1 : 250 or 1 : 500) in solution will do as well, and has the advantage of avoiding the shock caused by the curette or cautery. As far as possible, the room should be kept absolutely quiet, in order to avoid peripheral irritation. If possible the bedclothes should be supported by a frame above the patient, so as not to touch him, and of course must be light. Large doses of potassium bromide or chloral should be given by the mouth or rectum, either together or separately. To produce any effect, chloroform and ether would have to be given to full anesthesia, and they are of but doubtful value. Besides, as the patient has difficulty in breathing anyway, it is best not to further embarrass respiration. Curara seems to be indicated, and good results from its use are claimed by some writers. Large doses of opium are frequently serviceable, and digitalis, ammonia, or alcohol may be used as a stimulant.

Among drugs our chief reliance is upon physostigma. The success obtained by the administration of this remedy—a third of a grain subcutaneously, or a grain by the mouth, to be repeated in two hours—has been very gratifying. A more elegant preparation is physostigmine salicylate, by the administration of which subcutaneously the pain and spasm of efforts to swallow are avoided. "Tetanus antitoxine," discovered by Tizzoni and Cattani, has been used hypodermatically with success to cause immunity from the disease, even in highly susceptible animals. The blood-serum of these immune animals is antitoxic, and produces immunity to and cures the disease. Tetanus antitoxine may be obtained in solid form by adding alcohol to the serum of inoculated animals, and evaporating in vacuo.

The Secretary announced as the subject for the "Hunter McGuire Prize of One Hundred Dollars" for the session of 1893: *Obstruction to the Function of Micturition and the Results*. The offer is open to members of the State Medical Societies of Virginia, West Virginia, and North Carolina.

Dr. JOSEPH PRICE, of Philadelphia, Pa., announced that he would offer a *Prize of One Hundred Dollars for the Best History of Surgery and Surgeons in Virginia*, presented by any Fellow of the Medical Society of Virginia during its session of 1893, the conditions of award to be the same as those for the award of the "McGuire Prize."

SECOND DAY—SEPTEMBER 14TH.

DR. H. GREY LATHAM, of Lynchburg, Va., delivered the President's Address. His remarks were of special interest to the State.

The consideration of the subject selected for general discussion, "Vertigo," was taken up by DR. E. T. BRADY, of Marion, Va. He classified vertigo as stomachal, cardiac, cerebral, laryngeal (?), ocular, aural, toxic, and essential. He regards laryngeal vertigo as a form of epilepsy. He claims, in the majority of cases, that vertigo is of nervous origin, rather than due to abnormalities of the intra-cranial circulation. This nervous effect might be due to one of three causes: (*a*) The direct mechanical or chemical effect of poisons or imperfectly oxidized materials in the blood; (*b*) pressure upon centers governing equilibrium; and (*c*) acute localized inflammations, the equilibrial centers being disturbed by unusual impressions caused by and deflected from associated nerve-fibers. Attention was called to the fact that deaf-mutes are free from vertigo, as an indication that the sense of hearing entered largely into its causation. The use of cocaine locally in Ménière's disease was suggested.

DR. WM. C. DABNEY, of the University of Virginia, stated that vertigo is frequently due to the circulation of toxic principles in the blood, and that gastric and renal vertigo are thus to be explained. He recommended, in renal vertigo especially, the use of small doses of morphine—about a tenth of a grain—for temporary relief. Of course, the most important point in all cases is removal of the cause.

(To be concluded.)

CORRESPONDENCE.

INSANITY IN PRIVATE PRACTICE.

To the Editor of THE MEDICAL NEWS,

SIR: The list of works of reference on "Insanity in Private Practice," as given in my paper published in your issue of August 20th, is incomplete and not quite intelligible. I enclose herewith a corrected list of works, which, in justice to both of us, I would request to be printed as an addendum or correction of that which has already appeared.

Yours, truly,

KANKAKEE, ILL. RICHARD DEWEY.

"Sanity and Insanity," Mercier. New York: Scribner & Welford, 1890.

"Manual of Lunacy," Winslow. London: Smith, Elder & Co., 1874.

"A Manual of Psychological Medicine," Bucknill and Tuke. Philadelphia: Lindsay & Blakiston, 1879.

"Manual of Insanity," Spitzka. New York: E. B. Treat, 1889.

"Physiology of Mind," Maudsley. New York: Appleton & Co., 1878.

"Pathology of Mind," Maudsley. New York: Appleton & Co., 1882.

"Mental Physiology," Carpenter. New York: Appleton & Co., 1882.

"A Treatise on Insanity," Hammond. New York: Appleton & Co., 1883.

"Diseases of the Nervous System," Gowers. Philadelphia: P. Blakiston, Son & Co., 1888.

"Nerve Prostration," Roose. New York: Wm. Wood & Co., 1891.

"Familiar Forms of Nervous Disease," Starr. New York: Wm. Wood & Co., 1890.

"General Paralysis," Mickle. London: H. K. Lewis, 1886.

"Transactions of the Congress of American Physicians and Surgeons," vols. i and ii, published by the Congress. New Haven: 1889 and 1892, W. H. Carmalt, secretary, New Haven.

For readers of German: "Lehrbuch der Psychiatrie," Krafft-Ebing. Stuttgart: Enke, 1890.

For readers of French: "Clinique des Maladies du Système Nerveux," Charcot. Paris: Veuve Bebé et Cie, 1892.

"Études Cliniques sur les Maladies Mentales et Nerveuses," Falret. Paris: Ballière et Fils, 1890.

Also for general reference: "Manual of Human Physiology," Landois and Stirling; two vols. Philadelphia: Blakiston & Son, 1889.

"A Text-book of Pathology," Hamilton. London: Macmillan & Co., 1889.

COMPLICATION OR COINCIDENCE?

To the Editor of THE MEDICAL NEWS,

SIR: On the 8th of August, 1892, W. M., a young man, twenty-four years old, came to my office suffering from gonorrhea of moderate severity.

Under treatment with daily irrigations of the urethra with warm mild antiseptic solutions and appropriate internal medication, the urethritis almost disappeared, with no complications, at the end of two weeks. Then the patient complained of muscular pain in the right arm and shoulder, so severe as to render him unable to use the arm for any purpose. Examination disclosed spots of tenderness over the upper third of the biceps and middle of the anterior half of the deltoid. There was slight swelling and heat about the joint, but no stiffness or crepitus, and no pain on manipulation. There was, however, marked pain on active motion.

The temperature with the onset of the pain rose to 103° F. At the end of the first day this was reduced to 100° F. by the use of large doses of quinine.

The man was then given 10-grain doses of sodium salicylate three times daily. The shoulder was blistered by an application of cantharidal collodion. In four days the pain had disappeared, but the patient discovered that the power of abducting the arm was completely lost.

Examination showed complete paralysis of the deltoid, with some tenderness over its lower half.

There was no stiffness, or crepitus, or pain in the joint.

The patient was put on potassium iodide in 10-grain doses. There has been no improvement in the condition at the end of a week.

There was no history of traumatism.

Could the condition be due to gonorrheal infection, or was it merely a coincidence?

Respectfully, J. W. WILLIAMS, A.M., M.D.

PATERSON, N. J.

STEEL IN THE IRIS.

To the Editor of THE MEDICAL NEWS,

SIR: I have noticed of late reported in THE MEDICAL NEWS several cases in which particles of steel have remained imbedded in the iris for many years without causing trouble.

Perhaps the following case may be of interest in this connection. It occurred in the service of Prof. George E. Frothingham in the University of Michigan.

H. S. A., sixty-six years old, of Bay City, Michigan, entered the hospital October 16, 1885, with senile cataract in both eyes, the left mature, the right immature. In the iris of the left eye, in the outer inferior quadrant, near the pupillary border, was embedded a small, bright piece of steel, projecting forward into the aqueous chamber. The steel had penetrated the eye forty-eight years previously. On October 21st the lens of the left eye was extracted, local anesthesia being induced by means of a 4 per cent. solution of cocaine, the particle of steel not being disturbed. The subsequent progress of the case was uneventful. The patient left the hospital after the lapse of the usual time, with an excellent degree of vision, taking the piece of steel with him.

I have taken this case from my clinical notes while clerk of the ophthalmic clinic under Prof. Frothingham.

Respectfully, E. B. PATTERSON, M.D.

MICHIGAMME, MICH.

A CASE OF HEMORRHAGIC VARICELLA.

To the Editor of THE MEDICAL NEWS,

SIR: The following case seems of sufficient interest and rarity to merit publication. I am indebted to Dr. A. Trau, in whose practice the case occurred, for permission to record it:

W. S., a male infant, six months old, on July 25th, after slight indisposition, presented pearly vesicles on the face and body. Two or three other children in the family had just recovered from typical attacks of varicella. Dr. Trau saw the case and diagnosticated varicella. On July 28th there were numerous vesicles, especially about the face and head, which contained blood. Some of the clear vesicles were distinctly umbilicated. Crusts and scabs formed after rupture of the blood-containing vesicles, so that the child presented an unsightly and rather alarming appearance. There was considerable cerebral depression, at one time almost passing to coma. The highest temperature was 103° F. On August 3d the temperature was normal; the child was restless, and cried a good deal. There were no lesions upon the visible mucous membranes. More or less pitting is now perceptible as the result of the eruption, and for some days crops of pustules and furuncles appeared, especially about the head.

C. A. HAMANN, M.D.,
Surgeon Out-patient Department, German Hospital, Philadelphia.

EARLY PREGNANCY.

To the Editor of THE MEDICAL NEWS,

SIR: Thinking the following case of sufficient interest to warrant publication, I forward you the accompanying report:

On the morning of August 16th I was called in haste to attend a case in confinement five miles in the country. Upon reaching the bedside I found that I was a "little too late." The mother had been delivered of her child. The youthfulness of the mother attracting my attention, I learned, upon inquiry, that she was not yet fourteen years old. The history of the case revealed the fact that she was one of the number who had been unfortunate. The girl claims to have been assailed by some unknown person, and, in a helpless, unconscious condition, was ravished. As no information could be obtained by which the perpetrator of the deed could be identified, the child was born out of wedlock.

The young mother began to menstruate when she was about twelve and a half years old; three months afterward she became pregnant, and at full term was delivered as related. The child was well nourished, and normally developed except for its feet, both of which were deformed, being of the talipes varus variety, and of marked severity.

Respectfully yours,

L. PITNER WARRINGTON, M.D.

CHARITON, IA.

FOR SNAKE-BITE.

To the Editor of THE MEDICAL NEWS,

SIR: I have successfully employed the following mode of treatment in a number of cases of snake-bite. To the bitten place, by means of cloths, I apply the following every hour or two, or oftener if it seems necessary:

℞.—Acid. salicylic . . . . . ʒij.
Aquæ ammoniæ . . . . . ʒij.—M.

At the same time whiskey and ammonia are given freely. As soon as the line of demarcation is formed an incision is made through the skin between the dead and living parts. The parts are then washed with corrosive sublimate, 1:1000, three or four times a day, and the bite is dressed with 5 per cent. carbolized oil, which is kept constantly applied. Respectfully,

B. F. GOODLETT.

TRAVELLERS' REST, S. C.

STATE BOARD EXAMINATIONS.

To the Editor of THE MEDICAL NEWS,

SIR: At a meeting of the State Board of Medical Examiners of Washington, one of the Board, a "physiomedic," so-called, was the propounder of a list of questions on "Preventive Medicine." Among other things it was asked "What should be done if a person has eaten *anthrax meat?*" This was a puzzler to most of the applicants; but one man from the Middle States, of long experience and wonderful erudition, proved equal to the emergency, and gratified the examining member with this answer: "Swallow a turkey-buzzard, for he won't stop at anything."

Yours truly, C. W. S.

SEATTLE

NEWS ITEMS.

A. v. Hippel, of Königsberg, has succeeded v. Graefe in the occupancy of the Chair of Ophthalmology at Halle.

Bandl, a well-known gynecologist and obstetrician, died recently at Vienna.

Rokitansky, of Vienna, has been called to Graz to assume the duties of the Chair of Obstetrics.

John S. Billings, M.D.—At the recent tercentenary of the University of Dublin, the honorary degree of Doctor of Medicine was conferred, among others, upon Dr. John S. Billings, U. S. A.

An Emergency Fund for the Prevention of Infectious Disease.—The Philadelphia County Medical Society at its last meeting adopted the following preamble and resolutions:

WHEREAS, the appearance of Asiatic cholera in the commercial cities of Western Europe and the quarantine station of New York has excited great popular alarm, and whereas, recent events in connection with the quarantine at New York have illustrated how readily a conflict of authority might arise or popular fear be so preyed upon or legal form so employed as to hamper and nullify the efforts of those charged with the prevention of epidemics;

Resolved, That the Philadelphia County Medical Society expresses its belief that the introduction of Asiatic cholera and similar diseases into this country is strictly preventable, and that the only present danger of such occurrence lies in the inadequacy of the means at present provided for meeting such emergencies, and the possible legal or popular opposition to the efforts of those charged with administering our present quarantine regulations.

Resolved, That we urge the importance of placing at the disposal of the health authorities of the State a sufficient emergency fund for the thorough isolation and disinfection of infected persons or goods that may find entrance to the State.

S. D. Gross Monument Fund.—Dr. John B. Roberts, Treasurer of the American Surgical Association, acknowledges the reception of the following contributions to the Gross Monument Fund. Contributions are hereafter to be sent to the Gross Monument Committee, of which Dr. J. R. Weist, of Richmond, Ind., is Chairman.

Dr. Douglass Graham, Boston	$5 00
Dr. Solen Marks, Milwaukee	20 00
Dr. L. C. Lane, San Francisco	50 00
Dr. A. H. Plummer, San Francisco	20 00
Dr. C. Cushing, San Francisco	20 00
Dr. O. O. Burgess, San Francisco	20 00
Dr. Joseph O. Hirshfelder, San Francisco	20 00
Dr L. L. Dorr, San Francisco	20 00
Dr. James Simpson, San Francisco	20 00
Dr. Henry Palmer, Janesville, Wis.	10 00
Dr. Gibson, Janesville, Wis.	5 00
Dr. Mills, Janesville, Wis.	5 00
Dr. Pember, Janesville, Wis.	5 00
Dr. Whiting, Janesville, Wis.	5 00
Dr. Robert McLean, San Francisco	50 00
Dr. Francis L. Town, U. S. A.	20 00
Dr. Basil Norris, U. S. A.	20 00
Dr. G. L. Simmons, Sacramento	1 00
Dr. C. L. Simmons, Sacramento	1 00
Dr. N. R. Cluness, Sacramento	1 00
Dr. N. R. Cluness, Jr., Sacramento	1 00
Dr. C. Nichols, Sacramento	1 00
Dr. A. E. Brune, Sacramento	1 00
Dr. M. H. Woolsey, Sacramento	1 00
Dr. G. A. White, Sacramento	1 00
Dr. C. E. Fowler, Sacramento	1 00
Dr. W. H. Baldwin, Sacramento	1 00
Dr. M. Gardner, Sacramento	1 00
Dr. T. W. Huntingdon, Sacramento	1 00

The American Dermatological Association, at its annual meeting held in New London, Conn., on September 13, 14, and 15, 1892, elected the following officers: For President, Dr. George Henry Fox, of New York; for Vice-President, Dr. H. W. Stelwagon, of Philadelphia; for Secretary and Treasurer, Dr. Geo. Thos. Jackson, of New York; for Member-at-large of the Council, Dr. J. C. White, of Boston. The meeting adjourned to meet in Milwaukee on the first Tuesday of September in 1893.

CORRECTIONS.

In the second formula, in the first column of page 294 of THE NEWS of September 10, 1892, the quantity of alcohol should have read q. s. ad fʒv.

The figures 0.3 in the fourth line from the bottom of the second column of page 299 of THE NEWS of September 10th, should have read 0.003.